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ESSAYS

AND

OBSERVATIONS

ON THE

CONSTRUCTION AND GRADUATION

OF

THERMOMETERS,

AND ON THE

HEATING AND COOLING OF BODIES.

By GEORGE MARTINE, *M. D.*

The FOURTH EDITION.

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THIS NEW EDITION OF

DR. MARTINE'S ESSAYS

IS DEDICATED,

BY

THE EDITOR.

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ESSAY

ON THE

CONSTRUCTION AND GRADUATION

OF

THERMOMETERS.

Qui vel caloris vires, & calorem ipſum veluti in gradus partiri, vel materiæ cui inditus eſt copiam quantitatemque diſtincte percipere, &c.——*Utinam id allii & perſpicaciori præditi ingenio, & quibus in ſumma tranquillitate rerum naturam perſcrutari licuerit aſſequantur; ut homines non omnium modo ſcientes, ſed omnium fere potentes fiant.*

BERNARDINUS TELESIUS.

St. Andrews, Aug. 1738

SOME OBSERVATIONS AND REFLECTIONS CONCERNING THE CONSTRUCTION AND GRADUATION OF THERMOMETERS.

WE cannot enough commend and admire that excellent invention of *Thermometers*, whereby we are enabled to make ſome judgment of the various degrees of heat in bodies. It is not our buſineſs at preſent to determine to whom we owe that noble and uſeful diſcovery; whether to Sanctorio, to Galileo, to Father Paul, or to Drebbel: for I ſind it aſcribed to all theſe by their reſpective favourers and admirers[*]. At firſt, as is the fate of all other inventions, Thermometers were but rude and imperfect machines, and not eaſily to be applied to ſo many purpoſes as came afterwards to be in uſe. They were very clumſy; and, as the various degrees of heat were pointed by the different contraction or expanſion of air, they came afterwards to be found uncertain, and ſometimes deceiving meaſures of heat; as the bulk of air was affected not only by the difference of heat, but likewiſe by the difference of weight of the atmoſphere *.

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[*] The invention is given to Drebbel by his countrymen Boerhaave (Chem. I. 152, 156.) and Muſſchenbroek (Tent. Exp. Acad. Cim. Add. p. 8. Eſſ. de Phyſ. § 946.). Fulgenzio (Life of Father Paul, p. m. 158.) aſcribes it to his maſter Father Paul Sarpi, that great oracle of the republic of Venice. But there was an humour prevailed, in thoſe days, of fathering almoſt all the curious diſcoveries of the age on that great ſtateſman and virtuoſo. Vincenzio Viviani (Vit de l' Galil. p. 67. See too Oper. di Galil. Pref. p. 47.) ſpeaks of Galileo as the inventor of Thermometers. But we know how much and how fondly he adored the memory of his great maſter. (See Hiſt. Acad. des Scien. 1703, p. 169, 175, 176, 180). Still all theſe are poſthumous claims, and they too imputed by others. But Sanctorio himſelf (Com. in Galen. Art. Med. p. 736, 842. Com. in Avicen. Can. Fen. I. p. 22, 78, 219.) expreſsly aſſumes the invention in queſtion. And Borelli (de Mot. Animal. II. Prop. 175.) and Malpighi (Oper. Poſth. p. 30) aſcribe it to him without reſerve. And theſe Florentine academicians are not to be ſuſpected of partiality in favour of one of the Patavinian ſchool.

2. † Ferdinand II. great duke of Tuſcany, or the gentlemen of the *Academia de l' Cimento* under his protection, made a very great improvement on thoſe curious and uſeful machines. They made them with ſpirits incloſed in glaſs tubes, and theſe hermetically ſealed. So that they could ſuffer nothing by the evaporation of liquor, or the various gravity of the incumbent atmoſphere. And it was Thermometers made in this way which were firſt introduced into England by Mr. Boyle ‡, and came immediately to be of univerſal uſe among the virtuoſi in all the ſeveral countries, wherever polite learning and philoſophy were cultivated.

3. But there was not ſo much uſe made of thoſe inſtruments as they were capable of. We have plenty of obſervations made with particular Thermometers at different times and places. But then theſe were not conſtructed by any fixed ſcale or ſtandard. Even the Florentine Weather-glaſſes themſelves, whoſe higheſt term was adjuſted to the great ſun-ſhine heats of that country, were too vague and indetermined: and in other places every workman made them according to his own way and fancy, without adjuſting his numbers to any

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* Boyle Exp. on Cold, Abr. I. p. 577, *&c.*

† Vivian. Vit. de l' Galil. p. 67.

‡ Exp. on Cold, Abr. I. p. 582.

known or determined degrees of heat. And ſo they could not be compared one with another; nor could the obſervations made by different perſons, and in different parts of the world, be collated with any degree of certainty or juſtneſs By which means, notwithſtanding all the very numerous regiſters of the weather that have been kept and publiſhed by different authors, we are ſtill at a loſs to determine the comparative differences of heat and cold in different countries and climates; and the reſult of many other obſervations.

4. Had all the Weather-glaſſes in the world been made according to one determined ſcale, theſe inconveniencies and uncertainties would have been prevented; which are now unavoidable, and muſt ſtill continue ſo, until every body agree to graduate their Thermometers in the ſame way, or at leaſt determine ſome fixed or unalterable points of heat, to which all the different ſcales of thoſe inſtruments may be reduced.

5. The great and truly honourable Mr. Boyle* found himſelf very much at a loſs for a ſtandard; whereby to meaſure heat and cold: the common inſtruments ſhewing him no more than the relative coldneſs or heat of things, but leaving him in the dark as to their poſitive degrees. Whence he could not communicate the idea of any ſuch degree to

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* Exp. on Cold, Abr. I. p. 579.

another perſon. Thermometers were then ſuch indefinite and variable things, that it ſeemed morally impoſſible from them to ſettle ſuch a meaſure of heat and cold, as we have of time, diſtance, weight, *&c.* there having been then no method thought upon to compare together any two different Thermometers; or the obſervations made by them. Now as bodies are variouſly affected by various heats, regularly undergoing ſuch and ſuch changes, at ſuch and ſuch degrees of it, ſo the faireſt way to fix a ſtandard method of adjuſting Thermometers would ſeem to be from ſome remarkable change a body underwent by the application of a certain degree of heat to it. In conſequence of which, Mr. Boyle propoſed the freezing of the eſſential oil of aniſeeds as a term of heat and cold that might be of uſe in making and judging of Thermometers; and ſo to graduate them from this point according to the proportional dilatations or contractions of the included ſpirits. He mentioned, too, the coldneſs requiſite to begin the congelation of diſtilled water as another fixed term that might be propoſed; for he was perſuaded that among the ordinary waters ſome were apt to freeze more eaſily than others. But the objections which he apprehended might be made to this method ſcared him ſo much, that he proſecuted no farther this conſideration of fixing a ſtandard for making and graduating Thermometers all in the ſame way :—which it is pity one of his genius and induſtry, and aſſiſted with ſuch opportunities, did not carry farther; as it is of ſuch importance on a thouſand occaſions in the hiſtory and philoſophy of Nature.

6. The ingenious and acute Dr. Halley * was likewiſe very ſenſible of the bad effects of that indefinite way of conſtructing Thermometers; and wiſhed to have them adjuſted to ſome determined points. He, with Mr. Boyle, lays aſide the freezing of liquors, as being, in his opinion, points of heat not ſo juſtly determinable but with a conſiderable latitude. And what he ſhews the greateſt fondneſs for, is the degree of *temperature;* ſuch as in places deep under ground, where the heat in the ſummer, or cold in winter have been found to have no manner of influence. Thus in a cave cut ſtrait into the bottom of a clift fronting the ſea to the depth of 130 feet, with 80 feet of earth above it, Mr. Boyle † found the ſpirit in the Thermometer to be raiſed to the ſame height both in ſummer and winter. And Meſſrs Mariotte, De la Hire, and Maraldi aſſure us, that in the cave under the royal obſervatory at Paris, the heat continues always the ſame, ſcarcely altered by the moſt ſultry ſummer heats, or bittereſt colds of that country.

But, with Dr. Halley's leave, this degree of *temperature* I do not think a very convenient term for an univerſal conſtruction of Thermometers. Every body cannot go to Mr. Boyle's grotto: and it is but few who can have an opportunity of making obſervations, and adjuſting Thermometers in the cave of the Pariſian obſervatory. And we are not ſure that other deep pits would coincide preciſely with the temperature of theſe places. The difference of ſoils and different depths might occaſion a conſiderable variation.

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* Phil. Tranſ. Abr. II. p. 56.

† Mem. Hiſt. of the Air, Abr. III. p. 54.

7. Another term of heat Dr. Halley * thought might be of uſe in a general graduation of Thermometers, is that of *boiling ſpirit of wine.* "Only "it muſt be obſerved, that the ſpirit of wine uſed "to this purpoſe, be highly rectified; for otherwiſe "the differing goodneſs of the ſpirit will occaſion "it to boil ſooner or later, and thereby pervert "the deſigned exactneſs." He objects, too, its great aptneſs to evaporate, and that in length of time it becomes, as it were, effete, and loſes gradually a part of its expanſive power. But truly this laſt mentioned difficulty, though repeated after Dr. Halley by ſeveral others, and commonly pretty much inſiſted on, would ſeem to be of no great weight. Well rectified ſpirit of wine, if ſealed up in a glaſs, is pretty unalterable. It cannot evaporate: and by many years experience its force of expanſion has continued the ſame; as, beſide other obſervations, we know eſpecially from the annual regiſters of Mr. De la Hire's ſpirit Thermometer,

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* Phil. Tranſ. Abr. II. p. 35.

that has been kept in the obſervatory above theſe threeſcore years by-paſt.

8. But a much more convenient term of heat, though leſs inſiſted on by Dr. Halley, is that of *boiling water*. This he found * to be a very fixed and ſettled degree of heat; and which, when once water has acquired it, cannot be increaſed by any longer continuance or length of time. And this point of heat Sir Iſaac Newton and Mr. Amontons were juſtly very fond of in ſettling the limits of their reſpective Thermometers: wherein too they have been imitated by all thoſe that came after them.

9. Though I could ſcacely find any ſenſible difference in various waters, differing conſiderably in weight, ſaltneſs, cleanneſs, *&c.* ſo as that Mr. Taglini's objections † againſt the fixedneſs of this point are of no force; yet truly this heat of *boiling water* is not at all times and places ſo abſolutely fixed and invariable, as moſt people are ready to conclude from Dr. Halley's and Mr. Amontons's obſervations. The fixedneſs or volatility of water depends very much not only on the coheſion of its particles, but alſo on the preſſure of the incumbent atmoſphere;

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* Phil. Tranſ. Abr. II. p. 34. See, too, Amontons in Mem. Acad. Sc. 1699. p. 156. 1702. p. 210.

† See Mem. Acad. Sc. 1730. p. 714.

atmoſphere: * for, in a veſſel much exhauſted of air, water and other liquors, being freed of moſt of the ordinary preſſure they undergo, boil in a very moderate degree of warmth, much below what in the open air is uſually requiſite to ſet them a boiling. And, agreeable hereto, Fahrenheit actually found †, that water was capable of a greater or leſs degree of heat in boiling, according to the greater or leſs weight of the atmoſphere, or the greater or leſs height of the mercury in the barometer. But in ordinary changes of the weather, the difference is not very great‡. And farther, we may avoid all errors that might ariſe from any thing of that ſort, if we make our obſervations on the heat of boiling water, and adjuſt this term of heat at a middle

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[*] See Boyle Phyſico. Mech. Exp. Abr. II. p. 473, 474. Newton. Opt. p. 318.

[†] Phil. Tranſ. Abr. VI. 2. p. 18. Boerh. Chem. I. p. 171.

[‡] It is not ſo great as Dr. Boerhaave, (Chem. I. p. 171.) in giving an account of Fahrenheit's obſervations, reckoned. In an alteration of 3 inches in the height of the mercury in the barometer, he ſays, the boiling heat is found to differ 8 or 9 degrees. I did not find it ſo very much by the different weights of the atmoſphere. From the experiments I have made I judge that, by the riſe or fall of the quickſilver an inch in the barometer, the boiling heat of water varieth ſomewhat leſs than 2 degrees.

ſtate of the atmoſphere in places near the level of the ſea, when the mercury in the barometer ſtands at about 30 inches, or a very little under it. And the ſame caution will be neceſſary in judging of the heat of *boiling ſpirit of wine*, or of the *boiling heat* of any other liquor.

10. It was on this principle chiefly of the determined fixedneſs of the heat of *boiling water*, that Mr. Amontons*contrived his univerſal Thermometer. It was indeed a pretty contrivance: but it never was, nor never could be, of univerſal uſe; nor could it anſwer the deſign of its ingenious author. Its conſtruction is too difficult and complex; the machine itſelf is too bulky and incumbered to be eaſily managed, or carried from place to place. And then it is ſuppoſed to be fitted at a due temperature of the air, as in the ſpring and autumn at Paris: which, beſide that it is in itſelf ſomething too indefinite, at other times and places we cannot well be ſure of. And the air of that temperature he reckoned to be dilated juſt one third by the heat of boiling water. But the dilatation of the air is not ſo regularly proportional to its heat; nor is its dilatation by a given heat near ſo uniform as he all along ſuppoſed. This depends much on its moiſture†: for dry air does not expand near ſo

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* Mem. Acad. Sc. 1702, p. 204, *&c.*

† See Hiſt. Acad. Sc. 1708, p. 15. Mem. p. 370. Muſſchenbr. Tent. Exp. Acad. Cim. Add. p 40. Eſſ. de Phyſ. § 1402.

much by a given heat, as air ſtored with watry particles; which, by being turned into ſteam, increaſe vaſtly the ſeeming volume of the air. So then as Mr. Amontons's Thermometer is affected by all theſe and other inconveniencies [*], it is no wonder it was imitated by very few, and never came to be of general uſe in the world.

11. From this one determined point of the heat of *boiling water* there may, however, be laid down a general method of conſtructing Thermometers all in the ſame way, ſo as that they ſhall correſpond with one another in all the various degrees of heat. It is, as Mr. Boyle [†] propoſed long ago, by marking the degree of expanſion or contraction of the fluid of the Thermometers, as the heat applied is either greater or leſs than that of *boiling water*. Suppoſing, for example, the whole volume of this fluid to conſiſt of 10,000 parts, it is to mark on the tube where that volume is expanded by heat, or contracted by cold, 1, 2, 3 4, *&c.* of theſe parts. All which may be done at any time by different perſons and in different places, ſo as that they ſhall anſwer preciſely to one another.

12. This is a very plauſible method, and has been actually tried by ſome, as we ſhall afterwards have

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[*] See Mr. de Reaumur in Mem. Acad. Sc. 1730. p. 654.

[†] Exp. on Cold, Abr. I. p. 579.

occaſion to ſee, and yet in practice it will not be found very eaſy to determine exactly all the diviſions from the alteration of the bulk of the fluid; beſide other unavoidable inconveniencies and uncertainties we ſhall by and by have occaſion to take notice of. It is ſufficient, and will be much eaſier and more convenient, in ſettling an univerſal method of conſtructing Thermometers, to determine at leaſt two fixed points of heat at a conſiderable diſtance the one from the other, and to divide on the tube or ſcale the intermediate ſpace into any convenient number of equal parts or degrees. *Boiling water*, in the circumſtances we propoſed, we juſt now found to be a pretty uniform and fixed degree of heat. And *water* juſt *freezing*, if that come out always the ſame, will anſwer the purpoſe moſt readily of any degree we could think of.

13. It is true, Dr. Halley * thought this to be a point admitting a conſiderable latitude. And ſome have ſuſpected that water freezes at different degrees of heat in different ſeaſons, countries and climates. And Dr. Cyrilli's obſervations † would ſeem to confirm it. At Naples he found water to freeze when his Thermometer was 10 degrees above the freezing point, as it had been conſtructed in

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* Phil. Tranſ. Abr. II. p. 36.

† Phil. Tranſ. No. 424, p. 336. No. 430, p. 189. No. 434, p. 407, 408. No. 435, p. 464.

England by the directions of the Royal Society. And Father Martini * talks of the froſts in the province of Pekin in China as greater than its latitude of 42 degrees would induce one to expect, the rivers being often frozen for four months together, *&c.* And adds, that it is ſurpriſing the Europeans ſhould remain unaffected by this cold, and ſlight it as unable to produce ice in their quarter of the world. From all which one might be ready to conclude, with Dr. Derham †, Prof. Muſſchenbroek ‡, and others, that the farther north we go, it takes the greater degree of cold to freeze water. And what might ſtrengthen ſuch a ſuſpicion is, that I found ſome of the Dutch mercurial Thermometers made at Amſterdam, in which the *freezing point* was marked at *gr.* 32, to be a degree or two lower here when immerſed in thawing ſnow or beaten ice beginning to melt.

14. But I am convinced all this ſeeming difference flows from the careleſſneſs of obſervers, or errors of the workmen. Or what if Dr. Cyrilli kept his weather-glaſs ſhut up and ſheltered in a houſe while it freezed abroad? As I know it is but too ordinary with thoſe who pretend to keep regiſters of the weather. To ſatisfy myſelf in this article, I took a very

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*Atlas Sihenſ. p. 27. See, too, Boyle Ex. on Cold, Abr. I. p. 577. Reaumur in Mem. Ac. Sc. 1734. p. 233.

† Phil. Tranſ. ibid. ‡ Eſſ. de Phyſ. § 913.

fair method of inquiring if there was really any ſuch difference in the cold of *freezing water* in different climates. I marked two mercurial Thermometers here in the latitude of 56 deg. 20 min. and got a correſpondent of mine to mark ſome others at London in the latitude of 51 deg. 32 min. all at the freezing point in ſnow or powdered ice melting: and, upon exchanging theſe Thermometers, we found them, when again immerged in thawing ſnow or ice, ſtill to point at the very ſame marks preciſely, without the leaſt obſervable difference. Were there any odds occaſioned by the difference of climates, would not near 5 degrees of latitude have ſhewn it very ſenſibly*? And at this ſame very mark does the quickſilver ſtand in ſuch thawing ice or ſnow at all ſeaſons of the year, in ſummer or winter, in cold weather or warm weather, under a light or a heavy atmoſphere, *&c.* From whence I conclude the *freezing point* to be a very conſtant and ſettled degree of heat, more fixed and determined than even that of *boiling water*, and conſequently very fit to be one of the fixed limits in adjuſting our Thermometers.

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* The univerſality and fixedneſs of the term of congelation I can now affirm with ſtill greater confidence and certainty after the trials I lately made, not only at London, but at Paris in the latitude 48 deg. 50 min. and Dijon in the latitude 47 deg. 20 min. by which I found, that preciſely the ſame degree of cold was requiſite to freeze water in Burgundy and in the Iſle of France, as in our northern climates.

15. The fixedneſs then of the *freezing point*, I think we may take for granted, and ſufficiently eſtabliſhed by theſe obſervations, notwithſtanding what Profeſſor Taglini may have ſaid to the contrary; and the unneceſſary conceſſions Mr. De Reaumur thought fit to make on that head*; and the obſervations adduced by Dr. Muſſchenbroek†, by which he would prove the *freezing of water* to begin in various degrees of cold, depending on ſome ſaline additional mixture from the air. All which may be allowed to ſhew, that in ſome places and circumſtances water freezes more or leſs eaſily, though the degree of cold in this act of *natural freezing* be, for any thing we can perceive, in all pretty nearly the ſame.

16. The cold of *water beginning to freeze*, or, which is the ſame thing, of *ice beginning to melt*, is ſuch a convenient fixed point, ſuch a remarkable period of heat and cold, and withal ſo eaſily determined by emerging the bulb of the Thermometer in thawing ſnow or ice, that it is ſurpriſing ſo few have taken it into the ſcale of their Thermometers, that theirs might be the better compared

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* Mem. Acad. Sc. 1730, p. 712.

† Tent. Acad. Cim. Add. p. 184, 185. Eſſ. de Phyſ. § 913, *&c.*

with other peoples obſervations. We are not abſolutely ſure even of this part of the ſcale of the Florentin weather-glaſſes. And the French have not yet preciſely determined the *freezing point* in Mr. De la Hire's ſtandard Thermometer of the Obſervatory, by which the regiſter of the weather has been ſo very long kept; and with which they ſometimes compare other obſervations: and moſt of the other obſervations of the weather, both in the Memoirs of the Academy of Sciences and in the Tranſactions of the Royal Society, are done with Thermometers ſo negligently conſtructed, that we are left altogether uncertain of their degrees of heat: moſt of thoſe that have the *freezing point* marked on them having it very erroneouſly graduated, and alſo having no other determined or known period of heat; and ſo coming to be as indefinite and uncertain as the others.

17. The great Sir Iſaac Newton thought the ſettling the degrees of heat and cold well worth his notice; and, as he carried every thing he meddled with beyond what any body had done before him, and generally with a greater than ordinary exactneſs and preciſion, ſo he * laid down a method of adjuſting Thermometers in a more definite way than had been done hitherto. We have already † found

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* Phil. Tranſ. Abr. IV. 2. p. 1, *&c.*

† § 12.

it neceſſary, at leaſt convenient, to fix on two determined periods of heat to make up an intelligible ſcale of its various degrees. We have likewiſe* found *water freezing* and *water boiling* to be in two points of heat very convenient for ſuch a purpoſe. And theſe are the very degrees of heat he pitched upon. The liquor he uſed was Lintſeed Oil, a homogeneous fluid enough, and capable of a conſiderable rarefaction, and not having been obſerved to freeze even in very great colds, and able to bear a great heat without boiling. Suppoſing the bulb when immerged in *thawing ſnow* to contain 10,000 parts, he found the oil expanded by the heat of the *human body* ſo as to take up $\frac{1}{39}$ more ſpace, or 10,256 ſuch parts; and by the heat of *water boiling* ſtrongly 10,725; and by the heat of *melting tin* 11,516, beyond which he did not carry his oil Thermometer. So that, reckoning the *freezing point* as a common limit between heat and cold, he began his ſcale there, marking it *gr.* 0; and the heat of the *human body* he made *gr.* 12; and conſequently the heat of *boiling water* was expreſſed by *gr.* $34 = \frac{725 \times 12}{256}$; and *melting tin* by *gr.* 71.

18. I wiſh the world would have received this or any other determined ſcale for adjuſting their Thermometers. But I ſuppoſe they might be apprehen-

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* § 8, 14.

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ſive of ſome inconveniencies in this ſcheme. Beſide ſome inconſiderable arithmetical errors in Sir Iſaac's paper, and ſome few not accurately enough made obſervations of no great moment, they would alledge*, that he had expreſſed himſelf as if he had thought the point of *freezing water* to be the loweſt degree of heat. And indeed he has not mentioned any winter cold beyond that; though, as we ſhall take notice on another occaſion, we ſometimes experience vaſtly intenſer colds: in conſequence of which, it would be eaſy to protract Sir Iſaac's ſcale downward below *gr.* 0, or the freezing point, and ſo to make it as ſerviceable in common uſe, even for great degrees of cold, as other Thermometers.

But there is another difficulty which will hold in all oil Thermometers, or any made with a viſcid liquor, that it adheres too much to the ſides of the tube. In a ſudden cold or fall of the oil, a good deal ſticks by the way; and only ſinks gradually after, ſo that at firſt the ſurface appears really lower than the preſent temperature requires. And beſide, as at all times ſome muſt continue to ſtick and moiſten the inſide of the tube, in different degrees of heat and cold, the oil becoming alternately more or leſs viſcid, will adhere ſometimes more and ſometimes leſs; and therefore will inevitably diſturb the regularity and uniformity of the Thermometer.

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* See Amontons in Mem. Acad. Sc. 1703. p. 233.

19. The Florentin Thermometers made with ſpirit of wine were by no means ſo liable to errors of this kind. The ſpirit indeed muſt moiſten the inſide of the tube: but as this is ſo very thin a liquor, it is probable that moiſtening is always pretty uniform, and extremely thin; ſo as that it can have no other effect but to render the tube a very little narrower. And ſo truly rectified ſpirit of wine has been more uſed in Thermometers than any other liquor. It is very ticklish, is eaſily and very much affected by heat and cold; nor does it ſoon gather air bubbles, as watry fluids are ready to do even in no very great heats. But, as we ſaid before*, thoſe ſpirit Thermometers did not uſe to be reduced to a fixed ſtandard. At length Mr. De Reaumur† has in a very ingenious way attempted to eſtabliſh a general conſtruction of ſuch Thermometers, which might be copied at all times, and in all countries: and ſo to ſettle, as it were, a general correſpondence of obſervations that ſhould be made by ſuch inſtruments. He adjuſted his Thermometer to the *freezing point* by an artificial mixture: and then, immerging it in *boiling water*, found how much it was dilated by that heat, in the way Sir Iſaac Newton had managed his oil Thermometers. The quantity of ſpirits in his glaſs he ſuppoſed to be 1000 parts; and he found to the bulk of how

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* § 3, 4, 16.

† Mem. Acad. Sc. 1730. p. 645, &c.

many of theſe parts the liquor was dilated by that boiling heat. The ſtronger the ſpirits, this dilatation is found always the greater. The beſt ſpirit of wine commonly ſold, from this *artificial freezing of water*, to that *boiling heat*, was dilated $87\frac{1}{2}$ of thoſe parts [*]: and a mixture of ſuch a ſtrong ſpirit and water in equal proportions (which would be about the ſtate of common brandy) was expanded only $62\frac{1}{2}$ ſuch parts [†]. He ſpeaks too of a ſpirit ſo ſtrong as to have the dilatation of 90 in its *boiling heat* [‡]. But what he judged moſt convenient for his ordinary Thermometers was a ſpirit of ſuch a degree of ſtrength as in the above period of heat to be dilated juſt 80 of theſe 1000 parts [**].

20. This intimation of Sir Iſaac Newton with ſpirit of wine inſtead of oil is a promiſing enough method of making a ſettled ſtandard ſcale for Thermometers; and accordingly one of them conſtructed by Mr. De Reaumur himſelf is now placed in the Royal Obſervatory at Paris; and a journal kept of the weather by it. And others conſtructed in the ſame way have been ſent to different quarters of the world, to compare the degrees of heat of different climates: a project long ago of the great Colbert's [⸸], but never till now ſatisfactorily

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[*] Mem. Acad. Sc. 1730. p. 690. [†] Ibid. p. 692.

[‡] Ibid. 1734. p. 261.

[**] Ibid. 1730. p. 697, 698. and fig. 8.

[⸸] See Mem. Acad. Sc. 1702. p. 209.

executed. Obſervations have been made with them in the Torrid Zone. And the French Academicians had them in their philoſophical embaſſy to the north, for determining the figure of the earth. And yet I am afraid theſe Thermometers are conſtructed on principles, that will by no means be found ſo accurate as were to be wiſhed and expected in ſuch a caſe. Mr. De Reaumur * determines his freezing point, not from *thawing ſnow* or *ice*, but from an *artificial congelation of water* in warm weather. And as he † uſes very large bulbs for his glaſſes, it may be 3 or 4 inches in diameter, I am apt to think that, before the due temperature of the ſurrounding ice can be thoroughly propagated to the whole contained ſpirits, it will be quite melted down again; and ſo the *freezing point* marked much higher than what it ſhould be ‡. He indeed †† ſpeaks of a quarter of an hour as ſufficient to bring the contained ſpirit to the temperature of the ambient *medium*. But as far as I can judge from my obſervations (and many I have made on this ſubject), it would rather take ſome hours to penetrate thoroughly ſuch a great bulk of any liquor, and reduce it fully to its own degree of heat. And ſo Thermometers of great bulbs can never be uſed with ſucceſs, or truſted to in common experiments of the heat of bodies, or obſervations of the weather: as we may be ſatisfied by collating the ſeveral obſervations in the Royal Obſervatory made with De la Hire's and De Reaumur's Thermometers, which at different times I find to quadrate very ill together, juſt I ſuppoſe from that cauſe of the different ſizes of their bulbs *. For large bulbs can never be ſo quickly influenced by great and ſudden variations of heat and cold, as to make theſe changes, if they laſt but a ſhort while, ſufficiently remarkable, or to ſhew their real and full quantities. Small bulbs and ſmall tubes are (notwithſtanding the imaginary faults and difficulties ſtarted againſt them by Mr. De Reaumur †) vaſtly more convenient, and may be conſtructed ſufficiently accurate. I have them made with capillary bores that correſpond almoſt preciſely to one another in all degrees of heat; and conſequently muſt be allowed to have all the exactneſs that can be imagined or deſired in ſuch inſtruments.

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* Mem. Acad. Sc. 1730, p. 655, 656, 680, 681, 712.

† Ibid. p. 660, 710.

‡ From collating ſome obſervations together, I reckon Mr. De Reaumur's mark of freezing water, inſtead of coinciding with Fahrenheit's *gr.* 32, as it ſhould do, to correſpond with *gr.* 34. or a little above it.

†† Ibid. p. 711.

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* The diameter of the bulb of Mr. De Reaumur's I found to be about $3\frac{2}{3}$ inches; while the other was leſs than 2 inches.

† Mem. Acad. Sc. 1730. p. 650, 656, 659.

21. But if we cannot truſt to Mr. De Reaumur's *point of congelation*, what ſhall we ſay to his other fixed term, to wit, the heat of *boiling water?* This, I humbly think, is, under his management, ſtill more vague and uncertain. As water, were it ſurrounded by melted tin, though it would boil and foam, and ſuffer very great agitation, is yet incapable of being warmed beyond its ordinary great boiling heat, and continues always much colder than melted tin; juſt ſo, as I experienced upon trial, ſpirit of wine, though immerged in boiling water, can never acquire near ſuch a great heat, but keeps always a good deal under it, though bubbling and foaming, and toſſed to a very great degree. The ſpirit then in the Thermometer is abſolutely incapable of ſuch a great heat as Mr. De Reaumur aſcribed to it; and that not by a ſmall or trifling difference. I find highly rectified ſpirit of wine cannot be heated beyond *gr.* 175, or ſo, in Fahrenheit's Thermometer, while boiling water raiſeth the quickſilver 37 degrees higher: and common brandy was able to conceive a heat no greater than about *gr.* 190. So far was Mr. De Reaumur in the wrong when he thought that all ſpirits, weak and ſtrong, immerged in *boiling water* received a given degree of heat, and that equal to the heat of the ſurrounding water. I ſuppoſe his ſtandard ſpirit could take on a heat of about *gr.* 180; leſs by 32 degrees than what he reckoned.

22. Beſide all this, I truly think ſpirit of wine, though good enough for ordinary weather-glaſſes in temperate climates, is yet not ſo fit a fluid whereof to make ſtandard Thermometers. It is incapable of bearing very great heats, or very great colds. It boils ſooner than any other liquor; and though it keeps fluid in pretty ſtrong colds, yet it would ſeem, from ſome experiments, that it does not condenſe very regularly in them: and at Torneao near the polar circle, the winter cold was ſo violent, that the ſpirits were frozen in all their Thermometers *. So that the latitude of heat ſpirit of wine is capable of expreſſing, is by much too limited to be of very great or univerſal uſe.

23. What fluid then ſhall we take for our Thermometers? We have found inconveniencies in air, oil, and ſpirits; and water is more exceptionable than any of them. We have, it ſeems, nothing left but quickſilver. This is a very moveable and ticklifh fluid; it both heats and cools faſter than any liquor we know, or have had occaſion to try; faſter I am ſure than water, oil, or even ſpirit of wine; it never freezes by any degree of cold hitherto obſerved, and bears a great deal of heat before it arrives at a boiling expanſion, and, if well purified, does not wet or ſtick to the inſide of

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* See de Maupert. Fig. de la Terre, p. 58.

the tube. Dr. Halley [*], though appriſed but of ſome of theſe remarkable properties, thought it a fluid very fit for Thermometers, were but its expanſion more conſiderable. However, as any inconvenience ariſing from this conſideration may be avoided by making the bulb have a great proportion to the tube, ſo it is now very deſervedly come to be in the greateſt uſe in the Thermometers that people would moſt truſt to. It is ſaid [†], that they were firſt contrived by that curious mathematician Olaus Roemer. Mr. Fahrenheit in Amſterdam, and other workmen in that country, manufactured very many of them, and that in a portable and mighty convenient form for many purpoſes, making them very ſmall, and incloſing the tube in another glaſs hermetically ſealed. From Holland they were diſtributed into different quarters of the world, and have been imitated in ſome other places. And now they are made no where in greater perfection, or with greater exactneſs, than by our countryman Wilſon at London.

24. As Roemer's or Fahrenheit's ſcale is convenient enough, I wiſh it were univerſally kept and uſed, to preſerve an uniformity in our obſervations; and ſo that every body, when they ſpeak of experiments wherein the heat of bodies was adjuſted,

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[*] Phil. Tranſ. Abr. II. p. 34.

[†] See Boerh. Chem. I. p. 720.

or pretended to be obſerved by Thermometers, were to have the ſame language; and ſo be univerſally underſtood.

In this Thermometer the bulb is ſuppoſed to contain, as Dr. Boerhaave[*] and Dr. Muſſchenbroek[†] tell us, 11,124 parts of quickſilver, which ſtands at the loweſt mark, or *gr.* 0, in a very intenſe intolerable cold, being ſurrounded with a mixture of ſnow or beaten ice and ſal ammoniac or ſea-ſalt[‡].

If the ſame bulb be immerged in *ſnow* or *ice thawing* naturally, or in *water beginning to freeze*, the quickſilver is dilated, and ſo riſes in the tube 32 of theſe 11,124 parts; and therefore the ſpace of the tube from *gr.* 0 to the *freezing point gr.* 32, is divided into theſe 32 equal parts[||].

When the Thermometer is placed in water brought to ſtrong *boiling*, (the atmoſphere being in the condition formerly mentioned[**],) the quickſilver is dilated 212 of theſe parts beyond its original bulk of 11,124, ſo as now to poſſeſs in the bulb and tube together a ſpace equal to 11,336 ſuch parts: and the ſpace from *gr.* 32 to *gr.* 212, is divided into 180 equal parts or degrees of the Thermometer[††]; which, if the tube be long enough, may be protracted as far as is convenient. It may go well enough to *gr.* 600, and not much farther; for, with a heat but little greater than that, the mercury begins to boil.

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[*] Chem. I. p. 174.

[†] Eſſ. de Phyſ. § 948.

[‡] Fahrenh. in Phil. Tranſ. Abr. VI. 2. p. 52. Muſſchenbr. Diſſ. Phyſ. p. 680. Eſſ. de Phyſ. § 948.

[||] Fahrenh. in Phil. Tranſ. Abr. VI. 2. p. 52.

[**] § 9.

[††] Fahrenh. in Phil. Tranſ. Abr. VI. 2. p. 18, 52.

25. Dr. Boerhaave is not always quite conſiſtent with himſelf in his accounts of the number of parts into which the mercury in the bulb is ſuppoſed to be divided. In another place *, inſtead of 11,124, he reckons only 10,782, and afterwards † he raiſes it to 11,520, (which I take to be nearer the truth), and yet ſtill ſpeaks of a given heat as expanding the quickſilver the ſame number of parts as in the other ſupputation. Which of his ſuppoſitions is right, I am not abſolutely ſure of before making trial, nor need we much to care: for, without all that *apparatus*, I humbly think the eaſieſt and ſureſt way too, and what in practice I have followed, is not to be ſolicitous about the bulk of the quickſilver, but to fill the bulb and tube ſo, as that in freezing water, or melting ice, the mercury ſhall ſtand at a convenient height, which muſt be very nicely marked *gr.* 32; and then as accurately to obſerve where it ſtands when dilated by the heat of boiling water to *gr.* 212. The intermediate ſpace is then divided into 180 degrees; which ſcale may be protracted upwards and downwards as far as we ſhall judge convenient. In the conſtruction of

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* Chem. I. p. 165.

† Ibid. Expl. Tab. V. fig. 3.

theſe Thermometers the two great limiting or fixed points being *freezing water* and *boiling water*; which we found to be the moſt convenient periods of heat for adjuſting theſe inſtruments.

26. Indeed in all this we have ſuppoſed the bore of the tube to be perfectly cylindrical; which cannot always be obtained. But though it be tapering, or ſomewhat unequal, it is eaſy to manage that matter, by making a ſmall portion of the quickſilver, as much, for example, as fills up a half, or, if you pleaſe, a whole inch, ſlide backward and forward in the tube; and by this means to find the proportions of all its inequalities, and from thence to adjuſt your diviſions to a ſcale of the moſt perfect equality.

27. The conceiving the bulk of the contained mercury at the beginning of the ſcale to be either 10,782, or 11,124, or 11,520, or, as I gueſs it ſhould be, near 11,790 parts, and its expanſion from thence by the heat of boiling water to be 212 of theſe parts, was, to be ſure, a diviſion originally quite arbitrary. And I confeſs there might have been a more convenient one fixed upon at firſt, and adopted yet, if this were not now ſo univerſally known and uſed; and Thermometers conſtructed in that form every where ſcattered up and down, and in the hands of moſt of the Virtuoſi in Europe. I ſhall readily allow that Mr. De l' Iſle's at

Peterſburgh, might have had a greater look of ſimplicity. * In his the whole bulk of quickſilver, when immerged in *boiling water*, is conceived to be divided into 10,000, or rather 100,000 parts; and from this one fixed point the various degrees of heat either above or below it are marked in theſe parts on the tube or ſcale, by the various expanſion or contraction of the quickſilver in all the imaginable variety of heat. "As pure mercury," ſays he, "is of the ſame nature every where, nor is liable "to any alteration from being incloſed in a tube; "and as it is probable that, taking it equally puri-"fied, it will in different countries be ſubject to "the ſame expanſion, if expoſed to the ſame de-"gree of heat; for this reaſon he is perſuaded "theſe Thermometers may very well ſerve to "compare the temperature of different coun-"tries." And indeed ſuch a conſtruction bids fair for being a very good ſtandard method for graduating Thermometers all in the ſame way; and is much leſs liable to uncertainties than that ſimilar one of Mr. De Reaumur, whereof we have already given an account†. However, if Mr. De l'Iſle had pleaſed, I cannot but think it would have been rather ſomething better to have made the integer of 100,000 parts, or fixed point, at *freezing water*; and from thence to compute the dilatations or condenſations of the quickſilver in thoſe parts. All the common obſervations of the weather, *&c.* would have been expreſſed by numbers increaſing as the heat increaſed; which ſurely is the more natural way. Nor would there have been great incongruity or inconvenience in expreſſing, after the manner of Mr. De Reaumur, the few obſervations we have below ſimple freezing by numbers of contraction below *gr.* 0, or 100,000.

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* Phil. Tranſ. N. 441. p. 222.

† §. 19, 20, 21.

28. But, as I ſaid before*, however promiſing in theory ſuch a ſcheme may appear, it will not in practice be found very eaſy to determine exactly all the diviſions from the alteration of the bulk of the contained fluid. And beſide, as glaſs itſelf is dilated by heat, though in a leſs proportion than quickſilver, ſo that it is only the exceſs of the dilatation of the contained fluid above that of the glaſs that is obſervable; if different kinds of glaſs be differently affected by a given degree of heat, this will make a ſeeming difference in the dilatations of the quickſilver in the Thermometers conſtructed in the Newtonian method, either by Mr. De Reaumur's, or Mr. De l'Iſle's rules. Now Dr. Boerhaave†, from Fahrenheit's obſervations, ſpeaks of different kinds of glaſs as very differently affected by heat. And upon inquiry I was informed that Dr. Muſſchenbroek found by his Pyrometer, that

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* § 12.

† Chem. I. p. 141.

tubes of different ſorts of glaſs were variouſly affected, ſome more, ſome leſs, by the ſame degree of heat. From whence Thermometers conſtructed of ſuch different ſorts of glaſs would neceſſarily make the ſeeming dilatation of the contained fluid appear at different degrees in the very ſame point of heat. And accordingly Mr. Campbell, an ingenious and worthy member of the Royal Society, has, by experiments made with all imaginable exactneſs and ſcrupuloſity, found, in Thermometers conſtructed in Mr. De l'Iſle's way, the quickſilver to ſtand at different degrees of the ſcale when immerged in thawing ſnow. In ſome it was at *gr.* 154, in others at 156, and in another at 158: and emptying the tubes, and preparing them again, and that ſometimes with other mercury, he always, even in a great many trials, found the reſult come out the very ſame. So careful and exact had he been that the ſame tubes give always the ſame degrees of dilatation as nearly as could be expected in ſuch nice and ſubtile experiments. One of Mr. De l'Iſle's own Thermometers, which I tried very carefully, gave me always the quickſilver at about *gr.* 150. In another of his, where on the one ſide he has put Fahrenheit's ſcale, I ſee *gr.* 32 over againſt his own *gr.* 148½. In one conſtructed by Mr. De Monier at Paris, on De l'Iſle's principles, I ſaw the *terme de congelation* put down at *gr.* 148. Nay, by Mr. De l'Iſle's own accounts, I find his weather-glaſſes diſagreeing conſiderably from one another. So uncertain and precarious would this way of conſtructing Thermometers ſeem to be. From all which I humbly think that it is better and more convenient to drop this method altogether, and to fix on two points of heat at a conſiderable diſtance the one from the other; and to divide the intermediate ſpace of the tube into a convenient number of equal parts or degrees: as we juſt now propoſed * to be done in our imitations of Roemer's or Fahrenheit's Thermometers.

29. To enlarge our views of heat, and to enable us to compare other Thermometers with theſe, (which after this I wiſh were always to be the ſtandard), it would not be amiſs to obſerve a great many remarkable periods of heat and cold that have been taken notice of by others, and to determine where they fall on Fahrenheit's ſcale. We ſhould determine about what degrees on it the ſeveral ſorts of weather are marked; the various degrees of animal heat; what degrees of heat reduce ſuch and ſuch conſiſtent bodies into a fluid ſtate; the quantities of heat required to boil the fluids we have occaſion to try, &c. But the comparing the different Thermometers which have been in uſe in the world, and the determination of the various degrees of heat in bodies, make fruitful enough ſubjects for other curious and uſeful inquiries.

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* § 24, 25, 26.

THE

THE
COMPARISON
OF
Different THERMOMETERS.

LONDON, 1740.

AN ESSAY

TOWARDS COMPARING DIFFERENT THERMOMETERS WITH ONE ANOTHER.

WE had occaſion formerly to take notice of the great uncertainty of thermometrical obſervations, by reaſon of the vague and inconſtant way that people had of making their inſtruments. However, it will be worth while narrowly to inquire, as far as our lights can carry us, into the principles on which they were conſtructed; and if we can find out theſe principles ſo as to compare the old Thermometers with any regular one we are well acquainted with, we ſhall recover, as it were, the loſt obſervations of our predeceſſors; loſt for want of knowing the meaning of their numbers and graduations. And then obſervations made at different times and places by any known inſtruments will be no longer uſeleſs memorandums. We ſhall be able to compare them with one another, and with our own obſervations, and with the degrees of heat we are acquainted with.

2. As Sanctorio ſeems to be the firſt who had a notion of meaſuring the various degrees of heat by the dilatation of bodies, people will be apt to expect, and many will fondly wiſh, that he had left us his method of numbering the degrees of his Thermometers. Though, to acknowledge the truth, it would be a matter of more curioſity than uſe, as they were affected by the difference of weight of the atmoſphere, as well as by the difference of heat of the ambient medium. To remedy which inconvenience, the virtuoſi of the Cimentin Academy contrived their Thermometers in a better way; carefully ſealing up the included ſpirits from any influence of the gravity of the atmoſphere; and marking on the tubes, or on the ſcales fixed to the tubes, numbers whereby to judge of the various dilatations of thoſe ſpirits by the various degrees of the heat applied.

I am ſurpriſed to find Dr. Muſſchenbroek*, who had been at the pains to give a Latin tranſlation of their whole work, and to enlarge and embelliſh it with excellent additions, repreſenting theſe Florentine Thermometers as conſtructed in a very looſe, arbitrary and indefinite way, and without any rule at all. This is an imputation to which they are leſs liable than many others that came after them. They made them of ſeveral ſorts, ſome with greater and ſome with leſs accuracy. But in the conſtructing of thoſe in which they could moſt confide[*], they tell us expreſsly at what degree the ſpirit ſtood in the ordinary cold of ſimple ice or ſnow. Which, as their experiments were done within doors in Italy, I take commonly to have been in a thawing ſtate, and conſequently to point out to us the degree of freezing water, which we found[†] to be a very fixed period of heat and cold, coinciding, we know[‡], with our *gr*. 32. Thus in their 1ſt ſort this point of heat fell at *gr*. 20[§]; and in their 2d to about *gr*. 13½[‖].

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* Eſſ. de Phyſ. § 947. p. 5.

So then we have one fixed and intelligible point of heat in their Thermometers, which is more than we have in thoſe of many others. But from one point we cannot judge of the reſt of the numbers; and it muſt be confeſſed their higheſt degree of heat is not defined in a way we could have wiſhed. They tell us that in their firſt or long Thermometer the ſpirit, when expoſed to the great midſummer heats in their country, roſe to where they marked *gr.* 80[*]. And the ſpirit in the 2d or ſmaller Thermometer in ſuch a heat ſtood at *gr.* 40[†]. This indeed is a very incongruous way of graduating Thermometers, as the great heat of the ſummer ſun is ſuch an indefinite degree of heat in different days, years, climates, *&c.* And yet, by good chance, there is left us a way to judge of the particular ſummer heat they happened to pitch upon; and of finding whereabouts it would have fallen in a Thermometer conſtructed in Fahrenheit's way, which we have taken up as our ſtandard. To affirm this will, I know, be looked on at firſt ſight as pretending to a thing in itſelf loſt, and now impracticable. But Borelli and Malpighi, two curious and careful obſervers of nature, luckily had occaſion to tell us, that the *vicera* of ſome hot animals, ſuch as of cows, deer, *&c.* raiſed the ſpirit in the ſmall Florentine Thermometer to about *gr.* 40. the very degree of this their ſummer ſun heat. And that animal heat I find to coincide with *gr.* 102, or thereabouts in ours. From which two periods of heat determined in both Thermometers we can make a pretty good eſtimate of all obſervations made by any of the regular and well conſtructed Florentine glaſſes; as we may ſee by looking on the table at No. I. II. III.

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[*] Tent. Exp. Acad. Cim. p. 5.

[†] Eſſ. I. § 14.

[‡] Ibid. § 24.

[§] Tent. Exp. Acad. Cim. p. 2, 7.

[‖] Ibid. II. p. 129.

3. The Thermometers that came to be uſed in the world, were all imitations of thoſe of the *Aca-*

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[*] Tent. Exp. Acad. Cim. p. 3, 4. [†] Ibid p. 4.

demia de l' Cimento, but not conſtructed in ſuch a regular way, or upon ſuch determined principles. In France Mr. Hubin was employed in making them for the *Academie des Sciences* at Paris. But we find they were not made all perfectly alike. The Indian miſſionaries [*] tell us, that they uſed thoſe that were graduated loweſt. However, we may reckon at a medium that which Mr. Amontons gives us a view of in the Memoirs [†], under the name of the *Ancienne Thermometre.* In it the freezing point was at *gr.* 25, correſponding conſequently to Fahrenheit's *gr.* 32; and the heat of the cave of the Obſervatory was *gr.* 50, which I know to be our *gr.* 53. So that the compariſon is eaſy in the table at No. I. IV.

4. But I know not many obſervations made with this Weather-glaſs. It is much more to our purpoſe to know the conſtruction of that very remarkable one of Mr. De la Hire, that has ſtood in the Royal Obſervatory at Paris above theſe threeſcore years, and by which a regiſter of the weather has been conſtantly kept. And yet we cannot but regret that neither Mr. De la Hire himſelf, nor any of thoſe that ſucceeded him in that office, have been at the pains exactly to determine any two

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[*] Mem. Acad. Sc. 1666.——1698. VII. p. 835.

[†] Mem. Acad. Sc. 1703. p. 53.

points of it, ſo as that we may be enabled to imitate it; or compare it with other Thermometers; though I hope we ſhall be able to trace it pretty near the truth. We are expreſsly told, that the ſpirit in it always ſtands at *gr.* 48. in the cave of the Obſervatory *, correſponding by this to *gr.* 53. in Fahrenheit's. And when it freezes in the fields they ſpeak of their Thermometer ſtanding in the open tower as pointing at from *gr.* 30. to *gr.* 32 †: ſomething below which the real degree of freezing would be. And from ſome concurring circumſtances and obſervations I reckon this would fall to about *gr.* 28. correſponding to our *gr.* 32. as in the table No. I. V. We have an obſervation of Mr. De la Hire himſelf, whereby we find that his *gr.* 28. correſponded with *dig.* 51. *lin.* 6. in Mr. Amontons's Thermometer ‡.

5. Mr. Amontons made a fine ſtep to ſettle an univerſal Thermometer, had it not been for ſome inconveniencies of which we had occaſion to take notice formerly ||. In his the freezing point was at *dig.* 51. *lin.* 6. where our *gr.* 32 is; and the heat

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* Mem. Acad. Sc. 1700. p. 8. 1701. p. 10. 1702. p. 5. 1703. p. 3. 1704. p. 2. 1705. p. 4. 1706. p. 3. 1707. p. 2. 1708. p. 61. 1709. p. 3. 1710. p. 141.

† Ibid. 1702. p. 5. 1704. p. 4. 1705. p. 4. 1706. p. 3. 1707. p. 2. 1708. p. 62. 1709. p. 3. 1710. p. 141. 1711. p. 2.

‡ Mem. Acad. Sc. 1710. p. 142.

|| Eſſ. I. § 10.

of boiling water at *dig.* 73. where Fahrenheit's is at *gr.* 212: whence they can eaſily be compared together, as in the table No. I. VI.

6. * The Marquis Poleni gives us the reſult of a vaſt number of excellent and well-made meteorological obſervations, in which the heat and cold are meaſured with a Thermometer conſtructed in Mr. Amontons's way. But, as his numbers are conſiderably different, I thought it well worth while to give a ſcheme too of his Thermometer in the table at No. VII.

7. Mr. de Reaumur was very ſenſible of the difficulties of Mr. Amontons's conſtruction, and propoſed a new and more certain way, as he thought, of making Thermometers. He determined to begin his ſcale at freezing water, and at boiling water marking *gr.* 80, dividing the intermediate ſpace of the tube into ſo many equal parts. I have on another occaſion † given my reaſons for thinking he was in ſome miſtake with reſpect to both theſe two periods of heat. But, as many very curious and uſeful obſervations have been made with his Weather-glaſſes, it will be of ſingular uſe to find out the correſpondence of his ſcale with that of Fahrenheit. His boiling-water heat is really only the boiling heat of weakened ſpirit of wine, coin-

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* Phil. Tranſ. 421. p. 205.

† Eſſ. I. § 20, 21.

ciding nearly, as I gueſs, with Fahrenheit's *gr.* 180, and as his *gr.* 10½ is the conſtant heat of the cave of the Obſervatory[*], or our *gr.* 53, thence I find his freezing point, inſtead of anſwering juſt to our *gr.* 32, to be ſomething above *gr.* 34, as in the table No. I. VIII.

8. On principles very like thoſe of Mr. de Reaumur, Mr. de l'Iſle conſtructed his mercurial Thermometer: but, inſtead of freezing cold, he began his ſcale at the heat of boiling water; and, inverting the common order, marked downwards the ſeveral degrees according to the condenſations of the contained quickſilver; and conſequently by numbers increaſing as the heat decreaſed. This, as I obſerved[†], was not in all glaſſes ſo uniform as were to be wiſhed. But as in Mr. de l'Iſle's own ſtandards the freezing point is near to his *gr.* 150, coinciding with our *gr.* 32, it is eaſy to compare obſervations by them with the degrees of heat marked in ours by the table at No. I. IX.

9. Mr. Crucquius[‡] in Holland has made many curious and accurate meteorological obſervations with an air Thermometer. In freezing water the whole volume was 1070, in boiling water 1510;

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[*] Mem. Acad. Sc. 1730. p. 503.

[†] Eſſ. I. § 28.

[‡] Phil. Tranſ. N. 381. p. 4.

ſo as to render it very eaſy to collate his Thermometer with Fahrenheit's by the table No. I. IX.

10. The moſt common Thermometers in England are thoſe made (and commonly very careleſsly made) after the ſtandard one kept in the Royal Society: and many faſhioned on this plan, by order of the Society, have been ſent into foreign parts to eſtabliſh a correſpondence of obſervations concerning the weather in different countries: for which reaſon it is the more incumbent on us to find out the principles of its conſtruction, ſo as to enable us to compare it with other Thermometers. The ſcale begins, or *gr*. 0 is marked at the top, I know not well upon what grounds, and thence the numbers increaſe downwards as the heat decreaſes. * In it it is ſaid to be *extreme hot* about the top of the ſcale, *hot* at *gr*. 25, *temperate* at *gr*. 45, and *gr*. 65 is marked as the *point of freezing*. But, by trials made with ſome Thermometers that had been adjuſted pretty exactly with the ſtandard one in the Society-houſe, I found that the ſpirit fell to about *gr*. 78 or 79 in thawing ſnow; near 14 degrees lower than what had hitherto been reckoned: and this increaſes the wonder ſtill more, how Dr. Cyrilli † ſhould have found the freezing point at Na-

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* Phil. Tranſ. N. 429. p. 103. N. 433. p. 337, 339. N. 434. p. 407.

† Ibid. N. 424. p. 336. N. 434. p. 407.

ples ſo high as *gr.* 55, if his Thermometer was truly adjuſted to the ſtandard.

But, to compare this Thermometer with thoſe of other people, it is neceſſary to find out where they correſpond in ſome other period of heat. What is marked *hot* or *temperate*, *&c.* on it, is indeed very equivocal. But, to put an end to the uncertainty, I tried and found by experience, that *gr.* 34½ anſwered to *gr.* 64 in ours: from whence their agreement is eaſily determined for all the reſt of the ſcale as in the table at No. I. XI.

11. Sir Iſaac Newton * ſaw very well how vague and uncertain all the Thermometers in common uſe were; and therefore he contrived a new one, which I am ſurpriſed has gained ſo little ground in the world, as it is ſo ſimple and ſo eaſily imitated. You but begin your ſcale at freezing, and the diſtance from that to the heat of boiling water you divide into 34 equal parts or degrees, which, as they are large, may be ſubdivided at pleaſure. How ſuch a Thermometer anſwers to Fahrenheit's is ſeen at No. I. and XII.

12. There is a Thermometer in pretty frequent uſe in England, wherein they conceive the middle temperature of the air as neither hot nor cold, which therefore they mark *gr.* 0, and number both above and below, denoting by this means, as they

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* Phil. Tranſ. No. 270. p. 824.

conceive, the degrees of heat and cold of the ambient medium. It is Thermometers graduated in this way which they commonly have in their ſtoves and green-houſes for adjuſting the reſpective degrees of heat the various tender exotic plants require for maintaining their life and vigour. I do not believe that theſe Thermometers are made upon any regular or fixed principles. They commonly indeed mark freezing at *gr.* 30 under *gr.* 0. But in Mr. Fowler's regulator, who furniſhes moſt of the green-houſes with them, he, on my deſiring him to try it, found, that in melting ſnow the ſpirit fell to about *gr.* 34 under 0, and I found by ſome compariſons I made, that his *gr.* 16 coincided *q. p.* with our *gr.* 64: ſo that the whole correſpondence of this with Fahrenheit's Thermometer is eaſily determined, as in No. I. XIII.

13. Dr. Hales[*] thought fit to employ a new conſtruction of a Thermometer in making his curious experiments. He began his ſcale or loweſt degree at freezing, or our *gr.* 32, and carried it up to *gr.* 100, which he marked where the ſpirit ſtood when the ball was heated in hot water on which wax ſwimming firſt begins to coagulate. In ſuch water I found Fahrenheit's Thermometer point at *gr.* 142; by which the two Thermometers might eaſily be compared. But by experience his *gr.* 100 falls conſiderably above our *gr.* 142. The

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[*] Veg. Stat. p. 37.

reſult, from collating the two Thermometers together, is at No. I. and XIV. At a ſpring-head the Doctor found his to point at *gr.* 13, while ours ſtood at *gr.* 48.

14. At Edinburgh they have for theſe many years kept a very exact regiſter of the weather, part of which is already publiſhed in their Medical Eſſays: for which it is well worth while to find out its relation to other Thermometers; which, from what the authors tell us, is eaſily done: for, * when the bulb was immerged in melting ſnow, the ſpirit ſtood 8'2 inches high, the heat of the human ſkin raiſing it to 22'2 inches; ſo that the intermediate 14 was divided into inches and tenths of inches. And the heat of that perſon who graduated it I found to be *gr.* 97 in mine; from whence the compariſon at any intermediate degree of heat is very eaſy, as in No. I. XV.

15. We have heard of many other Thermometers, and of obſervations and regiſters of weather kept by them. But they have been generally ſo ill limited and deſcribed, that they are of no manner of uſe, and, to whatever purpoſe they might ſerve their authors, are to us as if they never had been.

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* Edinb. Med. Eſſ. I. p. 8.

ESSAY

ON THE

HEATING AND COOLING

OF

BODIES.

ST. ANDREWS, 1739.

ESSAY

ON THE

HEATING AND COOLING

OF

BODIES.

SUCH is the nature of Fire and Heat, that contiguous bodies of various degrees of warmneſs do naturally come to the ſame temperature, if inert and left to themſelves without any other intrinſic principle heating or cooling the one more than the other: the hotter body loſing, and the cooling one gaining ſome degrees of heat, till they all arrive at an *equilibrium*. Thus all inert bodies placed in our air at firſt heat or cool the particles next to them; but in proceſs of time they come all to be of the ſame temperature with it. See *Boerh. Chem.* I. p. 187, 188, 200, 281. *Muſſchenbr. Eſſ. de Phyſ* § 961. The times of bringing about ſuch effects, or reducing immerſed bodies to the ſtate of the ambient *medium*, being different, according to their relative bulks, denſities, figures, particular natures, &c.

2. A body very hot placed in a colder *medium* cools gradually, at firſt faſter and afterwards ſlower, as any one would expect; and is fully confirmed by Dr. Muſſchenbroek's experiments *. But he did not attempt to ſhew after what rule, or if it was after any regular manner that ſuch a cooling was effected. He only affirms † in general from them, that given quantities of heat are loſt at firſt in ſhorter times, and gradually longer and longer, till the cooling be very ſlow, and at laſt the heat of the body coincide with that of the ambient air.

3. Sir Iſaac Newton's ‡ view of this matter is vaſtly pretty, though not thoroughly underſtood by Mr. Amontons ‖. Having heated a lump of iron red hot, and with all the neceſſary precautions obſerved the degrees and times of the diminution of its heat when expoſed to the cold air, he ſuppoſed, and that not without a great ſhew of reaſon, that while it was cooling the quantities of heat loſt in given ſmall times would always be proportional to the heats ſubſiſting in it; reckoning the heat juſt

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* Tent. Exp. Ac. Cim. Add. II. p. 47—57.

† Ibid. p. 52.

‡ Phil. Tranſ. Abr. IV. 2. p. 3.

‖ Mem. Ac. Sc. 1703. p. 238.

to be the exceſs whereby it is warmer than the ambient air. So that from the beginning of its greateſt heat to the end or time when it came to the temperature of the ambient air, taking the times in an *arithmetical* progreſſion, the heats of the iron above the heats of the air, and the decrements of theſe heats will be *continually* proportional. For if there be a ſeries of quantities proportional to their differences, theſe quantities and differences will be in a geometrical progreſſion *. Whence the diminution of heat in ſuch a body is like the retardation of a projected body endowed with a certain originally impreſſed force, and moving in a medium with a reſiſtance always proportional to its velocity. And the progreſs of that decreaſe of heat may be determined by what is commonly demonſtrated of bodies reſiſted in that manner †.

4. This, I ſay, is a moſt ingenious way of finding out and determining thoſe very great and burning heats which no ordinary Thermometer can bear. But then it is not to be admitted without ſome reſtriction. The hypotheſis is more mathematical than phyſical. It gives a fine and beautiful, but not a true view of nature. The heat of a body does not really decreaſe exactly in that proportion.

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* Newton. Princip. II. Lem. 1. p. 230.

† See Newton. ibid. prop. 2. Varign. in Mem. Ac. Sc. 1707. p. 504, &c.

For were that truly the caſe, the body, though continually cooling, would take an infinite time to arrive at the temperature of the ſurrounding medium *. Which however, in fact, we find to be accompliſhed in a very moderate ſpace of time. And that ſooner or later according to the original heat, ſize, denſity, or other circumſtances of the heated body and cooling medium.

5. If you immerge a heated body in a cold air, or other cold *medium*, that body will acquire a ſort of atmoſphere involving it all around, ſomething warmer than the reſt of the air, though a great deal colder than the body itſelf: ſo as that the ambient cold cannot affect the body, ſo much as if it were quite naked and diveſted of its atmoſphere. But as this atmoſphere is warmeſt in or near the beginning, and gradually cooling along with the body, the ambient coldneſs will have a greater proportional influence towards the end than in the beginning. Whence, though the body in whole takes a longer time to cool, the differential decrements will be in a proportion ſomewhat greater than its inherent quantities of heat.

6. But what way ſhall we determine the law of this decreaſe, and how far it may deviate from the general and more ſimple law of thoſe decrements

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* See Varign. ibid. p. 507, 509.

being proportional to ſuch inherent quantities of heat? I will not inſiſt on any ſpeculation as ſufficient to lead us to this. But we may truſt to experience as our beſt guide. Obſervations carefully made, and judiciouſly applied, will undoubtedly bring us into the right path, or at leaſt very near it. Now, as the reſult of many obſervations, I find that the decrements of heat may be looked on as partly equable, and partly in proportion to the ſubſiſting heats; reckoning, as in the former hypotheſis, theſe heats to be the exceſſes whereby the heat of the body is warmer than the ambient air. And conſequently from the beginning to the end, taking the times in an arithmetical progreſſion, thoſe decrements may be reſolved into two ſerieſes. In the one, and that for ordinary of much the greateſt conſequence, they are always in proportion to the heats themſelves, and ſo in a geometrical progreſſion, as in the general theory: while in the other ſeries, indeed the leaſt material, the decrements are as the times, or always uniform, that is, equal quantities of heat loſt in equal times. And ſo the law of this decreaſe of heat coincides with the law of the retardation of a heavy body aſcending perpendicularly in a medium which reſiſts it in proportion to its velocity *.

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* See Newton. ibid. pr. 3. Varign. Mem. Ac. Sc. 1708. p. 175, *&c.*

7. To illuſtrate this by obſervation, I choſe chiefly to employ Dr. Muſſchenbroek's experiments; as having been carefully enough done, and not liable to the objection that might be ſtarted againſt mine, as if they had been made with a deſign to be ſubſervient to particular rules and theories. Though indeed it was the obſervations that led me to think of any ſuch rules taking place.

In a ſmall bar of Iron. Muſſchenbr. Tent. Acad. Cim. Add. II. p. 48.

The Times.	Degrees of Heat loſt, by obſervation.	Degrees of Heat loſt, by the firſt conſideration.	Degrees of Heat loſt, by the 2d conſideration.	Degrees of Heat loſt, according to calculation.	The Differences.
′					
1	62’5	59’4	1’2	60’6	—1’9
2	106	99	2’4	101’4	—4’6
3	128’6	125’7	3’6	129’3	+0’7
4	148’8	143’6	4’8	148’4	—0’4
5	161,8	155’6	6	161’6	—0’2
6	170’4	163’6	7’2	170’8	+0’4
7	176’5	169	8’4	177’4	+0’9
7½	180	171	9	180	0

In a ſmall bar of Steel. *Ibid.* p. 49.

′					
1	61’4	56’1	0’5	56’6	—4’8
2	94	95’2	1	96’2	+2’2
3	120	122’4	1’5	123’9	+3’9
4	141’5	141’4	2	143’4	+1’9
5	158’4	154’6	2’5	157’1	—1’3
6	167’7	163’8	3	166’8	—0’9
7	174’7	170’2	3’5	173’7	—1
8	179	174’7	4	178’7	—0’3
9	182’6	177’8	4’5	182’3	—0’3
10 q. p.	185	180	5	185	0

In a ſmall bar of Copper. *Ibid.* p. 50.

The Times. ′	Degrees of Heat loſt, by obſervation.	Degrees of Heat loſt, by the firſt conſideration.	Degrees of Heat loſt, by the 2d conſideration.	Degrees of Heat loſt, according to calculation.	The Differences.
1	76	71'4	0'8	72'2	−3'8
2	124'5	115'8	1'6	117'4	−7'1
3	141'2	142'2	2'4	144'6	+3'4
4	160'5	158'7	3'2	161'9	+1'4
5	171'6	168'8	4	172'8	+1'2
6	179'2	175'1	4'8	179'9	+0'7
7	184'4	178'9	5'6	184'5	+0'1
$7\frac{2}{15}$	185	179'3	5'7	185	0

In a ſmall bar of Braſs. *Ibid.* p. 51.

′					
1	120	114'1	1'5	115'6	—4'4
2	187	185	3	188	+1
3	232'6	228'7	4'5	233'2	+0'6
4	260'8	255'8	6	261'8	+1
5	280'2	272'6	7'5	280'1	—0'1
6	291	283	9	292	+1
7 q. p.	300	289'5	10'5	300	0

In a ſmall bar of Lead. *Ibid.* p. 51.

The Times.	Degrees of Heat loſt, by obſervation.	Degrees of Heat loſt, by the firſt conſideration.	Degrees of Heat loſt, by the 2d conſideration.	Degrees of Heat loſt, according to calculation.	The Differences.
′					
1	125	114	0’1	114’1	—9’9
2	177	174’6	0’2	174’8	—2’2
3	208	207	0’4	207’4	—0’6
4	225	224’3	0’5	224’8	—0’2
5	234	233’5	0’6	234’1	+0’1
6	239’1	238’4	0’7	239’1	0
7	241’7	241	0’8	241’8	+0’1
8	243’2	242’4	0’9	243’3	+0’1
8¾	244	243	1	244	0

In a ſmall bar of Tin. *Ibid.* p. 52.

′					
1	61’4	61	0’5	61’5	+0’1
2	96’6	96’2	1	97’2	+0’6
3	120	116’5	1’5	118	—2
4	132’7	128’1	2	130’1	—2’6
5	139’1	134’8	2’5	137’3	—1’8
6	142’4	138’7	3	141’7	—0’7
6⅘	144	140’6	3’4	144	0

In a little Mercurial Thermometer, heated to *gr.* 108, and expoſed to the open air, I found the heat decreaſe in this order.

The Times. '	Degrees of Heat loſt, by obſervation.	Degrees of Heat loſt, by the firſt conſideration.	Degrees of Heat loſt, by the 2d conſideration.	Degrees of Heat loſt, according to calculation.	The Differences.
0½	13	13'6	0'4	14	+1
1	25	24'1	0'8	24'9	—0'1
1½	34	32	1'2	33'2	—0'8
2	40	38'1	1'6	39'7	—0'3
2½	45'5	42'8	2	44'8	—0'7
3	49'5	46'4	2'4	48'8	—0'7
3½	52'5	49'1	2'8	51'9	—0'6
4	55	51'2	3'2	54'4	—0'6
4½	56'5	52'8	3'6	56'4	—0'1
5	58	54	4	58	0

8. The decimals of degrees are calculated and ſet down as they aroſe in the arithmetical reduction of the numbers: Not that I would be thought to pretend to an imaginary exactneſs either in my experiments, or thoſe copied from Dr. Muſſchenbroek. Meaſures and obſervations of this kind admit but of an approximation to geometrical truth. All our machines are imperfect. The conſtruction of Dr. Muſſchenbroek's Pyrometer made with toothed wheels,

pinions, *&c.* could not, as he himſelf confeſſes [*], move with ſuch a regularity as were to be wiſhed; and perhaps, too, the heated or cooled bodies are not quite homogeneous [†], and ſo ſometimes make the refrigerations go on in ſtarts: and accordingly in the regiſter of his obſervations we meet with ſtarts, ſtops, and irregularities, that in ſuch compound engines and imperfect bodies are unavoidable. And ſo we are to take the medium and main drift of the obſervations, in the main agreeing very well with the theory, though they may differ ſome degrees on one ſide or the other.

9. From this law of the decreaſe of heat in bodies expoſed to the influence of an equable ſtream of cooling air we ſhall be able with a tolerable degree of exactneſs to find out the intenſity of thoſe great burning fiery heats which cannot be meaſured by any Thermometers yet known. The heat of metals red hot, or even in fuſion, of burning ſtones, melted glaſs, *&c.* may by this means be determined. For when they are brought to the propoſed heat by a ſtrong fire, we are carefully to obſerve the time of their cooling; and by applying a mercurial Thermometer to them as ſoon as it can well bear their heat, we may accurately enough determine the decrements of heat below that period; and from thence, riſing upwards in a regular ſeries,

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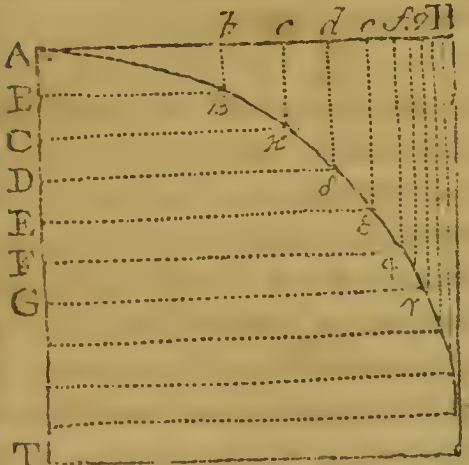
[*] Tent. Exp. Acad. Cim. Add. II. p. 29.

[†] See Ibid. p. 30, 31, 32.

be able to find out the degrees of heat from that to the very beginning of its cooling, or the moment it was taken from the fire.

10. I am very well aware that this law I have propoſed of the decreaſe of heat will not always hold exactly in all circumſtances. If the heated body be very large, and without a free ventilation of air, it will acquire a warm and almoſt ſtagnating atmoſphere around it, which increaſes and ſpreadeth wider for a conſiderable time after the beginning of the obſervation, and then gradually is diminiſhed and drawn into nothing at the end or expiration of heat. By which means it neither cools ſo faſt, nor in ſuch a regular manner, as if a conſtant briſk ſtream of freſh air were perpetually blowing along it. In thoſe circumſtances it will be difficult, if not impoſſible, to reduce the decreaſe of heat to a regular ſeries of numbers; however, even in this caſe, as well as the former more regular decline of heat, we may be able to find a method of tracing its ſeveral degrees in all its different periods of time, and exhibit them to the eye as diſtinctly, or rather more ſo, than if they were expreſſed in numbers; and that too in a ſort of geometrical way.

11. Let the time the body takes in cooling be expreſſed by the line AT, the quantity of heat loſt (being its exceſs above the heat of the ambient medium) by AH drawn perpendicular to it. AT being divided into any number of very ſmall equal parts AB, BC, CD, *&c.* gives AB, AC, AD, *&c.* the times from the beginning increaſing in an arithmetical progreſſion. And in AH the lines A*b*, *bc*, *cd*, *&c.* are ſuppoſed to expreſs the decrements of heat in theſe reſpective ſmall times. So that A*b*, A*c*, A*d*, *&c.* are the quantities of heat loſt from the beginning of the obſervation at any aſſigned moment of time, and HA, H*b*, H*c*, *&c.* the heats ſtill ſubſiſting at the reſpective times A, *b*, *c*, *&c.* Now from B, C, D, *&c.* draw the lines Bβ, Cκ, Dδ, *&c.* parallel to AH; and from *b*, *c*, *d*, *&c.* draw *b*β, *c*κ, *d*δ, *&c.* parallel to AT, ſo that they meet reſpectively, in β, κ, δ, ε, *&c.* and through theſe points of concourſe draw the curve A β κ δ ε, *&c.* this curve line, being



ing concave towards AT and convex towards AH will be the curve of the decreaſe of heat. AT may be looked on as its axis, AB, AC, AD, &c. as the abſciſſes, and Bβ, Cγ, Dδ, equal to *Ab*, *Ac*, *Ad*, as its ordinates. And I find this curve to be a ſort of hyperbola. So that from any given ſeries of obſervations of the decreaſe of heat of any body we ſhall be able to protract upon paper, or to calculate any other way, as much of this hyperbolic line, as to enable us to find its center, diameters, aſymptots, *&c.* and to draw it out to what length we pleaſe: and from thence to inveſtigate the heat of the body at any determined diſtance of time before we had acceſs to know the heat by a Thermometer. By which method then, only from obſerving the decrements of heat in a lump of iron, or any other intenſely hot body, after it is able to bear the application of a Thermometer, having taken particular notice how ſoon it comes to that temperature; we may determine its degree of heat the moment it was taken burning hot from the furnace. We may either calculate backward the ſeries of numbers found out by obſervation, or protract on paper mechanically the hyperbolic line or ſcale of the decreaſe of its heat in the manner juſt now propoſed. For either of theſe ways will give us the initial greateſt heat it had when taken from the fire with tolerable exactneſs. The former will be ſufficient, if the hot body be very ſmall, or a ſtream of cool air be conſtantly blowing along it; but the other will be more convenient if the ſurrounding air be in a ſtate of ſtagnation, or nearly ſo.

12. We ſee then from the firſt moment of the cooling of bodies the quantities of heat loſt in given times are always growing leſs and leſs; till the heat, and conſequently theſe decrements of heat, quite evaniſh, and the body put on the temperature of the ambient medium; and all this in a pretty regular manner. The heating of bodies does not go on in ſuch an orderly way; concerning which we have ſome very nice experiments furniſhed us by the ingenious and accurate Profeſſor Muſſchenbroek *: from which he juſtly infers, that the expanſion or heating of bodies is not equable, but very ſlow at firſt, as if the fire had ſome difficulty in its firſt penetration of the body which gradually grows eaſier, the expanſions or increments of heat in given times ſoon coming to be very conſiderable. Theſe in a very little time come to their greateſt height. And then ſuch increments gradually diminiſh till they quite evaniſh, the heated body acquiring the temperature of the ambient medium †. All which I have likewiſe found to be true by the experiments I have made on this ſub-

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* Tent. Exp. Acad. Cim. Add. II. p. 24—28. 34—43.

† Ibid. p. 29—32. 39.

ject. Some of which I ſhall ſoon have occaſion to inſert.

13. That bodies cool or heat ſooner or more leiſurely according to their different bulks, denſities, figures, &c. is commonly taken notice of. There will be no great labour required to convince every man that will but think of it, that a given quantity of the ſame ſort of matter will be eaſier or more difficultly cooled or heated, as it is expanded under a greater or ſmaller ſurface; and conſequently muſt vary according to the different figures wherein it is ſhaped. Thus bodies of a round form, as having the greateſt quantity of matter within the leaſt ſurface are, *cæteris paribus*, the ſloweſt in heating or cooling, and ſo on of the other figures of bodies, according to their compactneſs or expanſion.

14. As to their difference of ſize, no body makes any doubt of great bodies altering their temperature more ſlowly than ſmaller ones: "Quo "minor eſt corporis moles," ſays Lord Verulam*, "eo citus per corpus calidum approximatum in"caleſcit." But in what proportion to their bulk is not altogether ſo eaſy to determine. Let us, however, hear what Sir Iſaac Newton † ſays on the ſubject. "Globus major calorem diutius conſer-

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* Nov. Org. II. 13. § 40.

† Princ. Math. p. 509.

" varet in ratione diametri, propterea quod ſuper-
" ficies (ad cujus menſuram per contactum aëris
" ambientis refrigeratur) in illa ratione minor eſt
" pro quantitate materiæ ſuæ calidæ incluſæ.—
" Suſpicor tamen quod duratio caloris, ab cauſas
" latentes augeatur in minore ratione quam ea dia-
" metri: & optarim rationem veram per experi-
" menta inveſtigari."

15. To ſatisfy this great man's wiſhes, I began to make ſome experiments of this ſort, which I propoſed to have carried on farther if I had had leiſure enough. However, from the trials I made, I think we have good reaſon to conclude his ſpeculation of the faculties to preſerve heat being proportional to the diameters of ſimilar bodies, to be very juſt; and that his ſuſpicion of that faculty in the greater bodies, not being fully in ſuch a high proportion as the diameters, is without ſufficient ground.

16. I took two China bowls pretty nearly of the ſame ſhape but of very different ſizes, the large one being about twice as broad, and ſo of the other longitudinal dimenſions, as the other, and conſequently holding eight times more liquor. Into the big one A, I put two quarts, and into the other B two gills of water, which in both was brought to the temperature of *gr.* 112. by very nice Thermometers placed in each, and immediately removed

into a cool place, where there was a moderate current of air of the temperature of about *gr.* 48 or 49, and the refrigerations of the waters carefully obſerved in the following times.

Degrees of Heat.	Times in A.	Times in B.	
		By obſervation.	By theory.
	′	′	′
gr. 112	0	0	0
104	10½	5½	5¼
96	21¼	11	10⅝
80	54½	28	27¼
72	81	42	40½
64	118	61	59
56	194	99	97

The ſame day the experiment was tried over again, but in a place where the ſurrounding air had no current, and little other agitation than what was occaſioned by the motions I made in carrying on the obſervations. The reſult was as follows:

Degrees of Heat.	Times in A.	Times in B.	
		By obſervation.	By theory.
	′	′	′
gr. 112	0	0	0
104	10	6	5
100	16	9	8
96	23	12½	11½
92	31	16	15½
88	40	21	20

The Thermometer in the little bowl was ſomewhat above *gr.* 112 in the beginning, which was the cauſe of the times of the falling to ſuch and ſuch degrees of heat to be always about a minute longer than the theory required. There was ſomething, though leſs of this too, in the foregoing experiment: and, making a due allowance for theſe ſmall and truly inconſiderable variations, the theory coincides ſurpriſingly with obſervation; nearer indeed than we could well expect in ſuch experiments, wherein the inſtruments muſt be allowed not to come up to a mathematical exactneſs. A very ſmall and inſenſible error in the conſtruction of the Thermometers, or a very inconſiderable miſtake, as of about a quarter or half a degree in making the obſervation, would ſeemingly occaſion a very ſenſible difference between the time actually obſerved and that which the theory required.

To try it in bodies differing ſtill more in bulk from one another, the heat of the air being *gr.* 48, there were two veſſels filled, the one with one gill and the other with 27 gills of water, their reſpective diameters thus being 1 and 3; and the refrigerations came out in this order.

Degrees of Heat.	Times in A.	Times in B	
		By observation.	By theory.
	′ ″	′ ″	′ ″
gr. 108	0 00	0 : 00	0 : 00
104	5 : 20	1 : 30	1 : 45
100	10 : 00	3 : 20	3 : 20
96	15 : 45	5 : 05	5 : 15
92	22 : 10	7 : 10	7 : 23
88	30 : 00	9 : 00	10 : 00
84	39 : 00	11 : 15	12 : 20
80	48 : 20	14 : 00	16 : 07
76	57 : 00	17 : 30	19 : 00
64	101 : 00	33 : 10	33 : 40

There ſhould be more experiments tried, and theſe on other bodies as well as water; on ſolids as well as fluids, and in a greater variety of proportions of their reſpective bulks, and with bodies immerſed in various mediums, *&c.* all which I am apt to think would come out, like to what we have already tried, agreeable to Sir Iſaac's rule.

17. Let us next inquire if the ordinary theory of bodies heating and cooling faſter or ſlower, as they are leſs or more denſe, will come out as conformable to nature and truth. It is indeed a very plauſible ſpeculation, and what one would be apt to deduce from the *vis inertiæ* of matter, that rare bodies are ſoon either heated or cooled, but that denſe bodies are more ſlowly altered by the heating

or cooling influence of the ambient medium. "[*] Suſpicamur (naturam denſi & rari) poſſe habe- "re conſenſum cum tarda & celeri exceptione & "depoſitione calidi & frigidi. Fiat igitur experi- "mentum, ſi rarius corpus non admittat, & amit- "tat calorem aut frigus celerius, denſius vero tar- "dius."—"Quo denſiora corpora," ſays Dr. Boerhaave[†], "ſive fluida fuerint ſive conſiſtentia, "eo pluri tempore egent, ut ab eodem igne æqua- "liter incaleſcant." And in like manner, as to the cooling of bodies, he tells us[‡], "Corpora— "acceptum caloris gradum tanto diutius conſer- "vare quo ſunt denſiora, ponderoſiora, aut plus "ſubſtantiæ corporeæ habentia." In a word, "Quo denſa magis corpora eo diutius impreſſi ca- "lidi tenacia[‖].——Tanto pluri tempore egent ut "poſſint redire ad temperiem cum rarioribus citius "refrigerandis[**]."

This rule of bodies requiring times of heating and cooling in the order and proportion of their denſities, is, I ſay, a very plauſible ſpeculation, and to which every body at firſt hearing is ready to aſ-ſent: and moreover this propoſition, ſo obvious in theory, is ſaid to be confirmed too by experiments, as we find them propoſed by Dr. Boerhaave*, "Multa ſuper hac re cogitanti" (ſays he) "*obſervatum* certe id fuit, eo citius caleſcere ab eodem "igne corpus quo rarius fuerit; eo lentius autem "refrigerari calefactum ſemel, quo denſius fuerit, "eo citius refrigeſcere quo rarius erat. Si vas aëre "plenum, aliud aqua, utrumque dein eidem calo-"ri exponitur, erit forte aër ſic calidus millies ra-"rior aqua quoque ita calida, ſed aqua conceptum "tanto tardius calorem tanto diutius retinebit, ut "aër forte millies citius refrigeſcat.—† Sit vas ca-"vum, parallelepipedum, ſupra apertum, ex aëre, "aqua repletum; cui imponantur vaſa cylindrica "vitrea, æqualia; repleta ad eandem altitudinem "diverſis pondere liquidis; dein ſupponatur ignis, "ut aqua intra hoc vas aſſidue mota æquabiliſ-"ſime incaleſcat, cernemus nudo oculo liquidum "livius, adeoque rarius, citiſſime expandi, den-"ſius vero multo lentius; quin & thermoſcopia "impoſita idem docebunt. Caleſcit ocyſſime aër, "dein Alcohol, Oleum Petrolei liquidiſſimum po-"ſtea, tum Oleum Terebinthinæ, mox aqua pura, "dein aqua ſalſa, Lixivium fortiſſimum, Metal-"la, Mercurius, Aurum." And, as to their cooling, he gives us a ſimilar experiment, and with a like ſucceſs. "‡ Si enim in aqua ebulliente diver-"ſa pondere corpora æqualiter inde caleſcunt,

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[*] Bacon Hiſt. Denſ. &c. p. 17.

[†] Chem. I. p. 279. vid. & p. 266.

[‡] Ibid. p. 264.

[‖] Ibid. p. 160.

[**] Ibid. p. 267.

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* Chem. I. p. 200.

† Ibid. p. 279.

‡ Ibid. p. 264.

" id diutiſſime calidum manebit quod ponderoſiſſi-
" mum, id citiſſime friget quod leve. Quoſque
" autem hactenas per experimenta procedere datur
" regula fere generalis hæc habetur; vacuum Tor-
" ricellianum calorem in eo genitum una momento
" amittit. Aër calefactus in olla, citiſſime calorem
" conceptum perdit. Alcol lentiore gradu. A-
" qua diutius quam Alcol. Argentum Vivum tar-
" dius vero eodem calore refrigeratur. Ita inter
" ſolida quoque, lignum, ſaxum, metella, iterum,
" equaliter calefacta, retinent queſitum calorem
" exacte tanto ſerius." This ſcheme of bodies having a faculty of preſerving their temperature in proportion to their reſpective denſities, is illuſtrated from ſundry conſiderations by Dr. Muſſchenbroek *, who reckons them up in this order †, air, alcohol, petroleum, oil of turpentine, rapeſeed oil, diſtilled vinegar, water, ſalt water, aqua fortis, oil of vitriol, ſpirit of nitre, quickſilver.

18. I know no ſtronger inſtance than this of the weakneſs, or, if I may venture to ſay ſo, of the preſumptuouſneſs of the human underſtanding, in pronouncing too haſtily concerning the nature of things from ſome general preconceived theories. That rule of bodies acquiring given degrees of heat or cold, *cæteris paribus*, in times juſt reciprocally

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* Eſſ. de Phyſ. § 969.

† Ibid. § 944.

proportional to their denſities, though plauſible enough in theory, I cannot find to be true in fact. Air, indeed, is heated and cooled ſooner than any other fluid I have had opportunity of trying, though not in ſuch a different proportion as its extreme rarity, compared with the great denſity of other fluids, would require. And water is ſlower of heating and cooling than either oil or alcohol: but, contrary to all our fine theory, quickſilver, the moſt denſe ordinary fluid in the world, excepting only melted gold, is, however, the moſt tickliſh next to air; it heats and cools ſooner than water, oil, or even rectified ſpirit of wine itſelf.

Taking about 15 ounces of mercury, and $\frac{1}{14}$ of this weight of water, ſo that their volumes were pretty equal, I put them into two very thin glaſſes of the ſame ſize and make, with a Thermometer in each: and after letting them ſtand a good time in the cold air, till both Thermometers pointed preciſely *gr.* 46, I ſet them down at the ſame time, almoſt cloſe to one another, before a great fire, at a conſiderable diſtance from it, but ſo that the heat ſhould equally act upon them. And in the following times the Thermometers (which in themſelves were very like one another, and equally tickliſh) ſhewed the reſpective fluids to be heated in the following order;

Times.

Times.	Water heating.	Mercury heating.
'		
0 or beginning.	*gr.* 46	*gr.* 46
3	51	56
6	57	66
9	62	73
12	66	78
15	70	82
18	73	84
21	76	86
24	77½	86½
27	79	87
30	80	87
33	80½	87
36	81	87
39 The fire ſtirred.	82	88
42	84	90
45	85	92
48	87	93 -
51	87½	93+
54	88	93+
57	88½	93+
60	89	93

Then the water and quickſilver being both brought to the ſame temperature of *gr.* 59, were at the ſame time ſet down in an open window, where the temperature of the air was at *gr.* 52; and the two Thermometers ſhewed the fluids to cool in this order:

Times.

Times.	Water cooling.	Mercury cooling.
′		
0 or beginning.	*gr.* 89	*gr.* 89
3	85½	81½
6	82	76
9	79	72
12	76	63
15	73½	65
18 A blaſt of air.	70½	61½
21	68	59½
24	66	58
27	64½	57
30	63	56
33	62	55½

Here we ſee that water, inſtead of acquiring or loſing a given quantity of heat 13 or 14 times ſooner than quickſilver, as it ſhould do by the common theory, is about twice ſo ſlow in heating and cooling than what mercury was found to be. Could the glaſſes have been made any thinner, perhaps the mercury would have appeared to cool ſtill ſomething more quickly. And as the fire did not act equally all the while, you find, too, ſome little anomalies occaſioned thereby.

As this experiment differed ſo widely from the common opinion, and from what every body, to whom I have had occaſion to propoſe it, expected, I tried it over again with the ſame cautions, but with larger volumes, taking near 48 ounces of mercury, and 3½ ounces of water; their quantities being to one another as 13½ to 1; and ſo their volumes pretty equal. And the reſult of the obſervation was as follows:

Times.	Water heating.	Mercury heating.
′		
0 or beginning.	*gr.* 48	*gr.* 48
4	52	58
8	57½	69
12	63	78
16	68	84½
20	72½	89½
24	77	92½
28	80	95
32	83	96½
36	85	97
40	87	98
44	89	99
48	91	100
52	92½	100
56	93½	100
60	94	99½

They were both reduced to the heat of *gr.* 92, and then carried into the air of the temperature of *gr.* 51, and cooled in the following order; all the while, the water evaporating a little, and ſticking in drops to the ſide of the glaſs.

Times. ′	Water cooling.	Mercury cooling.
0 or beginning.	*gr.* 92	*gr.* 92
4	88	85½
8	85	80
12	82	76
16	79½	72½
20	77	69½
24	75	67
28	73	64½
32	71¼	62½
36	69½	61
40	68	59½
44	66¾	58¼
48	65½	57
52	64½	56¼
56	63¾	55¼
60	62¾	54½
64	61¾	54
68	61	53½

19. As oil is more tenacious than water, many are apt to think it will be ſlower both in admitting the particles of heat and fire, and in cooling or letting them go. "Omnes Philoſophi," ſays Dr. Boerhaave*, "cenſerent olei tenacitate irretitum "ignem longe diutius in oleo hæſurum." And another theory will lead them to a new opinion: when they conſider that oil, conſiſting chiefly of ſulphureous particles, will therefore, from their notion of the Newtonian obſervations of the rays

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[*] Chem. I. p. 263.

of light and heat, be more apt both to attract and retain the heat applied to it. But how weak and fallacious are all our ſpeculations concerning the nature of things not thoroughly well underſtood! Dr. Boerhaave, in direct oppoſition to this laſt theory, makes water both heat and cool more ſlowly than oil; and that exactly in proportion to their reſpective denſities. * "Bina vaſa æqualia "accepi, quorum unum aqua, oleo implevi oliva- "rum alterum. Ambo repoſui in vaſe, in quo "feci ebullire aquam, retinui in illa ebulliente aqua, "donec certus eſſem utroſque liquores eodem bul- "lientis aquæ gradu æque calefactos; exemi tum "ex eo vaſe, repoſui in eodem ambo aëre, ut vi- "derem tempus, quo uterque liquor reducebatur "ad eundem refrigerationis gradum; atque inve- "ni, pro ratione ponderis comparati, prorſus æ- "quale." Nor this did I find exactly true. Oil did indeed both heat and cool faſter than water; and, contrary to all the theories, even much faſter than its difference in ſpecific weight ſhould have made it. Oil of olives is but about $\frac{1}{10}$ lighter than water: but it is heated and cooled faſter in a much greater proportion. That exceſs is not indeed always the ſame, or near the ſame. It does not ſhew ſuch an uniformity of difference as quickſilver did. In the beginning there is no great difference; then water takes about $\frac{1}{4}$ or ſo more time in acquiring

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* Chem. I. p. 263.

or loſing given quantities of heat than what the oil does: but in the progreſs of the experiment, the difference becomes always greater and greater, till towards the end the water is almoſt twice as ſlow in heating and cooling as oil, taking the times from the beginning of the obſervation; as in the following experiments:

Times. ′	Water heating.	Oil heating.
0 or beginning.	*gr.* 50	*gr.* 50
8	54	54
16	63	66
24	71	78
28	75	83
32	79	88
36	82	93
40	85½	96½
44	88	99½
48	91	102
52 the fire decaying.	94	105
56	96	107
60	99	109
64	101	110½
68	103	111½
72	104	111½
76	104½	111½

And both being brought to the temperature of *gr.* 108, they were ſet in the air of between *gr.* 50 and 51, and they cooled in this order:

Times.

Times. ′	Water cooling.	Oil cooling.
0 or beginning.	*gr.* 108	*gr.* 108
4	104	102
8	100	96
12	96½	90
16	94	86
20	91½	81½
24	88½	77½
28	86	74
32	84	72
36	82	70
40	81	68
44	79½	66½
48	78	65
52	76½	63½
56	75	62½
60	74	61½
68	71½	60
76	69½	58½
84	67½	57½

The experiment of the heating and cooling of oil was repeated with the following ſucceſs:

Times,

Times. ′	Water heating.	Oil heating.
0 or beginning.	*gr.* 45	*gr.* 45
4	45½	46
8	49	54
12	52½	55½
16	57½	62
20	61½	69
24	65	74
28	68½	78½
32	71½	82½
36	74½	86
40	77	88½
44	79	90½
48	81	93
52	83	95
56	84½	96
60	86	97
64	87	98
68	88	98½
72	89	99
76	91	100½
80	92	102
84	93½	103
88	94½	103½
92	94¾	103¾
96	95	104

Both the water and oil were brought to the heat of *gr.* 96, and placed in a large room, the temperature of which was about *gr.* 45; and they cooled in this order:

Times,

Times. ′	Water cooling.	Oil cooling.
0 or beginning.	gr. 96	gr. 96
4	92	90
8	89	85
12	86	80
16	83½	76½
20	81½	73
24	79½	70
28	77½	67½
32	75½	65½
36	74	63½
40	72½	61½
44	71	60
48	69½	58½
52	68	57½
56	67	56½
60	66	55½
68	64½	54
76	62¾	52¾
84	61¼	51¾
92	59¾	51
100	58½	50½
116	56	49
132	54	48
148	52½	47½
180	50¾	46½
212	49	45½
244	47¾	45¼
276	47	45
340	46	45

20. The heating and cooling of water and rectified ſpirit of wine came out as follows:

Times. ′		Water heating.	Sp. wine heating.
0	or beginning.	gr. 52	gr. 52½
4		53	54
8		56½	59½
12		61	65½
16		65	71
20		69	76
24		73	81
28		77	85
32		80	88
36		83	91
40		85½	93½
44	The fire decaying.	88	96
48		90	97½
56		92½	99½
60		94	100
64		95	100

They were both brought to the heat of *gr.* 95½, and expoſed to the open air of the temperature of *gr.* 57 or 58, where they cooled in this order:

Times.

Times. ′	Water cooling.	Sp. wine cooling.
0 or beginning.	*gr.* 95½	*gr.* 95½
4	91½	90½
8	88	86
12	85	82½
16	82½	79
20	80	75½
24	78	73
28	76	71
32	74	69
36	72½	68½
40	71	67
44	70	66½
48	69	65½
52	68	64
68	65	62
84	62½	60½

Thus we ſee that ſpirit of wine both heats and cools faſter than water, and that in a much greater proportion than the inverſe *ratio* of their ſpecific weight does require, as we obſerved likewiſe of oil. But ſtill quickſilver, however denſe, is more tickliſh and eaſier affected by heat and cold than any of theſe fluids. Common brandy, upon trial, I did not find to differ ſenſibly from water in this reſpect.

21. The ſame theory, drawn I ſuppoſe from the general doctrine of the *vis inertiæ* of matter, led

Dr. Boerhaave* and Dr. Muſſchenbroek † to lay it down as a principle, that the faculty of cooling other bodies was in proportion to the denſity of the cooling medium. "Corpora," ſays Boerhaave, "quæ ignem jam in ſe continent copiâ majore, "quam ambientia fluida, vel vicini corpora, illum "ignem amittunt eo citius, quo in fluidum denſius "immittuntur refrigerandi cauſa. Quod ita intel-"lectum velim. Sit aër, aqua, argentum vivum, "in vaſis, ejuſdem accurate temperiei in omnibus "his. Eſto tum igniti ferri fruſtum triplex æquale "perfecte ignitum. Unum horum trium relin-"quatur in aëre notatæ temperiei, immergatur "ſecundum in aquam accurate tam frigidam quam "aër hoc tempore. Tertium vero intrudatur in "argentum vivum etiam æque frigidum ac prio-"res, aër et aqua. Quid fiet? in raro aëre "ferrum diu retinebit ſuum calorem, in aqua ci-"tius amittet, in argento vivo citiſſime. Et qui-"dem videtur in illa aqua fere tanto refrigerari "citius, quanto hæc aëre denſior; adeoque oc-"tingenties ocyus. In argento vivo forte quater-"decies citius, quam in aqua."

22. Neither does this ſpeculation come out true in fact. The times of bodies cooling in theſe different fluids do by no means differ ſo much as is commonly imagined. Inſtead of 800, a body cools

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* Chem. I. p. 267.

† Eſſ. de Phyſ. § 567.

but about 8 times faſter in water than in air. And the refrigerating virtue of quickſilver was found inſtead of 14 times greater than that of water, to be a very ſmall matter ſo, as about 2 ſeconds or ſo in a minute.

Air, water, and quickſilver, being all brought to the common temperature of *gr.* 52, I placed ſucceſſively in theſe ſeveral fluids a ſmall Thermometer that had always been heated to *gr.* 112; and carefully obſerved the times wherein it loſt 56 degrees of heat, or wherein the mercury ſunk to *gr.* 56 on the ſcale, which was within 4 degrees of the heat of the ſurrounding *mediums.* And by four different trials the refrigerations came out thus:

	′ ″	′ ″	′ ″	′ ″
In Air -	8:45	8:30	8:20	8:25
In Water -	1:04	1:03	1:04	1:04
In Quickſilver	1:00	1:03	1:01	1:02

23. This doctrine of the heating and cooling of bodies, in all its ſeveral branches, ſhould be carried on by a variety of experiments, which I have neither leiſure nor opportunity of proſecuting at this time. So that I muſt leave it to others, whoſe ſituation is more favourable for making the neceſſary obſervations for eſtabliſhing and perfecting that doctrine, to which I have only been able to furniſh ſome helps.

AN

ESSAY

TOWARDS A NATURAL and EXPERIMENTAL HISTORY OF THE VARIOUS DEGREES of HEAT in BODIES.

lower kind, and altogether diſtinct from the heat of animals. And this, too, they diſtinguiſhed into natural and preternatural, or morbid, as ſorts of heat quite different from one another. And thoſe, too, they reckoned of different natures in the different ſpecies of animals. Doctrines and ways of ſpeaking of this ſort, ſet up by the Peripatetic ſchool, and too much adopted by Galen and the phyſicians after him, continued long in the world; and were alſo countenanced by the chymiſts, theſe *philoſophi per ignem*, who profeſſed and valued themſelves on a more than ordinary knowledge of the ſecrets and operations of heat.

2. Gomez Pereira, who was among the firſt that dared to think freely in philoſophy as well as phyſic, ventured to affirm * all heat, whether of an elementary, igneous, or animal kind, to be of one and the ſame nature; that the heat of the ſun, or any other elementary heat, did not differ from that of animals but in degree; and that the heat of a man in a fever differed only by its exceſs from that of a man in health; and that the heats of other animals did not differ ſpecifically, but only in quantity, from one another, or from that of the human kind.

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* Nov. Med. cap. ii. p. 7, 9. cap. iii. p. 12, 17, 20, 23, 24, 25.

3. The learned Teleſius, though he was ſenſible of the great variety of the degrees of heat in bodies, thought that the determining them and aſcertaining their differences to be almoſt impoſſible. So that he declines taking the trouble of any ſuch inquiry. "Qui calor," ſays he *, "vel quantus, "& quæ ejus copia, & quæ entia in qualia invertat minime inquirendum videtur, ut quod homini nulla, ut nobis videtur, innoteſcere queat "ratione. Quî enim vel caloris vires, & calorem "impſum veluti in gradus partiri, vel materiæ, cui "inditus eſt, copiam quantitatemque diſtincte percipere, & certis determinatiſque caloris viribus "copiæque in certam materiæ quantitatem, dipoſitionemque certas actiones; & certæ materiæ "quantitati certam determinatamque caloris copiam aſſignare liceat?"

The purſuit however of all this he recommends to others, as being in itſelf of very great conſequence. "Utinam id alii & perſpicaciori præditi "ingenio, & quibus in ſumma tranquillitate re"rum naturam perſcrutari licuerit aſſequantur; "ut homines non omnium modo ſcientes, ſed omnium fere potentes fiant." So fond would he have been of the ways of meaſuring heat found out ſince that time. Though for his own ſhare he abſolutely deſpairs of doing it, eſpecially with any tolerable accuracy. "Nobis ut ingenue fateamur

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* De Rer. Nat. I. 17.

"craſſiore ingenio donatis, & quibus non niſi extremum vitæ ſpatium philoſophari licuerit, minimeque id moleſtiis, curiſque vacuis—ſatis ſit, ſi qui quantuſque calor, quam molem, qua donet diſpoſitione intueri liceat Et non exquiſite id quidem, diſtinctèque, ſed rudiore & confuſo quodam modo, nec ſingulas caloris differentias, quæ innumeræ & roboris & copiæ ſunt; nec materiæ conſpiſſationes diverſitatemque percipere potentibus, ſed perpaucas utriuſque & confuſe illas, indiſtinctèque."

4. The nature of heat my Lord Verulam ſeems to have conſidered with more than ordinary attention; and gives his doctrine *de forma calidi* *, by way of example for managing philoſophical ſubjects. And he found all the various heats not to differ in kind, but in their various intenſities, durations, and other modifications and accidents †.

5. Sanctorio's Thermometer, given out to the world about that time, ſhewed all kinds of heat to have a ſimilar effect in dilating the air; according to whoſe various expanſions, the various intenſities of thoſe heats, of whatever kinds, were to be judged. The ſame was ſoon found to hold in

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* Nov. Orig. II. 11, 12, 13, 18, 20.

† Ibid. p. 189, 196, 197, 200, 206, 208, 220, 252, 253, 254.

other fluids. And at length the firmeſt bodies themſelves were, by undeniable proofs, found to ſwell by the application of heat of any ſort. And from the various degrees of thoſe dilatations alone we muſt take our meaſures of the various degrees of heat in bodies.

6. But where ſhall we begin to reckon the heat of bodies, or their greateſt natural contractions, or the limits of heat and cold? Many things, when firſt propoſed, look ſingular and odd, that, after laying aſide prejudices, and by mature conſideration become familiar and eaſy; and of the truth of which we come at length to be fully ſatisfied. We commonly conceive water, when once frozen and turned to ice, to have loſt all heat, and ſo to have become abſolutely cold; on which account many philoſophers did too haſtily begin their reckonings of heat from this point. But as various fluids, ſuch as melted wax, melted tallow, oil, wine, brandy, *&c.* loſe their fluidity at various degrees of heat, and after that may ſtill grow colder and colder in their firm ſtate; juſt ſo, after water begins to freeze, we know from many conſiderations it can become ſtill much colder. That is, that in freezing water there is ſtill a conſiderable quantity of heat; which by degrees grows leſs and leſs, till the cold becomes intolerable to us mortals; as in the dead of winter in the northern intemperate climates, where men cannot live without

more than ordinary ſhelter from the extreme ſeverity of the weather. The air is oftentimes as much colder than water beginning to freeze, as ſuch water is colder than our ſummer weather. We reckon it warm when the fluid in the Thermometer is raiſed to *gr.* 64, it is at *gr.* 32 when water freezes; and ſuch colds have been often ſeen as to bring it down to *gr.* 0, the beginning of the ſcale, nearly the cold produced by a mixture of ſnow and ſalt.

7. Not as if this was the loweſt degree of heat, or the greateſt degree of cold that nature has ever been obſerved to produce on our earth, as Dr. Boerhaave * ſeems to think, and which he †, though I know not on whoſe teſtimony, tells us was the greateſt cold that was obſerved in Iceland in the cold winter of $170\frac{8}{9}$. The cold has been ſometimes felt as great, or near as great as this in climates ordinarily temperate enough, and certainly much warmer than what we have reaſon to believe the bleak cold country of Iceland to be. At Germanopolis in Pennſylvania, only in the latitude 40°, *an.* $173\frac{1}{2}$, the cold brought the mercury to *gr.* 5 ‡. In the famous winters $170\frac{8}{9}$ and $171\frac{5}{6}$, the cold at Paris was ſuch, that it would have

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* Chem. I. p. 162, 164, 166, 399, 400, 593.

† Ibid. p. 158, 166, 174.

‡ Act. Berolin. Cont. IV. p. 130.

brought down the mercury in our Thermometer to *gr.* 8[*]. At Leyden, *an.* $172\frac{8}{9}$, it ſunk to *gr.* 5[†], and at Utrecht it fell a diviſion lower[‡]. At London, in the winters $170\frac{8}{9}$, and $173\frac{0}{1}$, the cold was ſo violent as to bring the ſpirits almoſt down to the artificial cold of an ice and ſalt mixture[‖]: and *an.* $170\frac{8}{9}$ Mr. Roemer[**] at Copenhagen, in the latitude of 55 deg. 43 min. found the mercury in his Thermometer to fall fully thus low, to wit, to *gr.* 0[††].

8. But if we go farther north, though not ſo far as Iceland, we ſhall meet with much greater colds than any thing ever ſeen in theſe countries. In

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[*] Mem. Acad. Sc. 1710, p. 186. 1717, p. 3.

[†] Boerh. Chem. I. p. 158.

[‡] Muſſchenbr. in Phil. Tranſ. 425. p. 359.

[‖] Derham in Phil. Tranſ. Abr. IV. 2. p. 113. VI. 2. p. 50.

[**] Vide Boerh. Chem. I. p. 720.

[††] Dr. Boerhaave ſpeaks of this remarkable obſervation of Mr. Roemer as made at Dantzick. But I rather think it to have been made at Copenhagen. Dr. Derham (ſee Phil. Tranſ. Abr. IV. 2. p. 114.) had papers in his hands giving an account of this very froſt *an.* $170\frac{8}{9}$ at Copenhagen, ſaid to be taken from the obſervations of Mr. Roemer: and Mr. Roemer was a Dane, lived and bore high offices at Copenhagen, and died there *an.* 1710. However, in the main, this does not at all alter the ſtory with reſpect to us.

January 1732 the cold was ſo great at Upſal, that it brought the ſpirit of wine in one of the Royal Society's Thermometers down to *gr.* 124, which falls about a degree below *gr.* 0 in Fahrenheit's. And at Peterſburgh in the latitude only of 59° 56′, much the ſame with that of Upſal, *an.* 173$\frac{2}{3}$, the cold was ſo exceſſive, that quickſilver was by it contracted $\frac{1}{50}$ part of the bulk it had in boiling water, or to fall to *gr.* 200 in De l'Iſle's Thermometer *, which brings it to —*gr.* 28 in ours. We know in general, from the accounts of travellers, what intenſe colds rage in winter in many places of the world, and eſpecially in or near the frigid zones in both hemiſpheres. But we have few obſervations on record exactly limited or reduced to a determined meaſure. However, in M. de Maupertius's † accurate journal, we find that the French academicians, who, in purſuit of knowledge, and to determine that important queſtion of the figure of the earth, wintered *an.* 173$\frac{6}{7}$ at the north polar circle, experienced a degree of cold much greater than any thing we find meaſured and recorded by others. At Torneao, in the latitude of 65° 51′, even on this ſide that circle, their ſpirit of wine Thermometers came at length to be frozen, and a mercurial one conſtructed in Mr. de Reaumur's way ſunk to *gr.* 37 under his freezing point, which would have been about 33 diviſions below *gr.* 0 in Fahrenheit's. When that intenſe cold was ſuddenly admitted into their warm rooms, their bodies could hardly ſuffer it, their breaſts were as if they had been rent, and the moiſture of the air was in an inſtant converted into whirls of ſnow. This is a degree of cold as much below the cold of freezing water, as that is below the ordinary heat of our ſkin: for it is 33+32=65 diviſions below the freezing point at *gr.* 32, and the heat of the human ſkin at *gr.* 97, is juſt ſo much above it.

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* Phil. Tranſ. 441. p. 222.

† Fig. de la Terre, p. 58.

9. To find a cold ſo far below that of freezing water is certainly very ſurpriſing: and yet a greater than this almoſt inconceivable degree of cold, may by art be procured in leſs rigid climes: for we can contrive a method of cooling much beyond the ordinary tendency and courſe of nature. Many things when mixed together immediately become vaſtly colder than formerly, when they ſubſiſted diſtinct from one another. Spirit of nitre poured on beaten ice or ſnow brings on a very intenſe cold. Theſe, when their temperature and that of the air was but about *gr.* 32 above *gr.* 0, the ingenious and induſtrious Fahrenheit, by an artful management, cooled ſo much that the mercury in the Thermometer ſunk to −*gr.* 40, or 72 diviſions below the freezing point*; a degree of cold great-

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* See Boerh. Chem. I. p. 162. Muſſchenbr. Tent. Exp. Acad. Cim. Ad. p. 174.

er than what was felt in the neighbourhood of the polar circle: and this is as much below the cold of freezing water, as that is below the heat of our hotteſt animals, or of men in fevers. No body would have thought, *a priori*, that freezing ice was capable of ſuch an additional quantity of cold.

10. Nor are we abſolutely ſure that yet in theſe circumſtances all heat was deſtroyed. Even in that horrid and moſt bitter cold at Torneao, which the French mathematicians mention with ſo much horror, we are ſure there was; and likewiſe even in that exceſſively cold mixture of Fahrenheit there might ſtill be, and probably there was, ſome degree of heat. What if there was a concourſe of ſuch frigorific circumſtances? If Fahrenheit's experiment was to be made in the frigid zone, what would be the effect, and what a dreadful cold might by that means be produced? * So then, for want

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* When I was lately at Paris there were put into my hands ſome very extraordinary obſervations, communicated to the Royal Academy of Sciences by Mr. De l'Iſle at Peterſburgh, of exceſſive colds at Kirenga in Siberia, a place not very far north neither, lying in the latitude only of 58°, 10'. There in the winter $173\frac{7}{8}$ the mercury once fell to *gr.* 275 in De l'Iſle's Thermometer, which by calculation I find ſhould be *gr.* 118 below the beginning of the ſcale in ours. This, if there be no miſtake in the affair, is the moſt aſtoniſhing

of knowing the ultimate limits of heat and cold we cannot determine the geometrical proportion of the real or abſolute quantity of the heat of one body to that of another. All we ſhould pretend to, is to aſſign their arithmetical differences; which is a very uſeful work, and is to be done by a great number of accurate obſervations on the various expanſions of bodies by the various degrees of heat; theſe expanſions always correſponding in ſome meaſure to the quantities of fire or heat applied. So that we ſhall in the ordinary way ſpeak of theſe things as proportional to one another. Though I would not be underſtood poſitively to affirm that they both go on always preciſely in the ſame ratio; or that the expanſions were always exactly in a ratio neither greater nor leſs than the ſimple ratio of the quantities of the applied heats.

11. When people * tell us that red-hot iron is 3 or 4 times hotter than boiling water, and that boiling water is about 3 times hotter than the heat of our ſkin, or of the ſummer heat, they generally reckon the loweſt limits of heat to be where wa-

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ing thing I ever met with of the kind. It ſuppoſes a cold 150 degrees below the freezing point, almoſt an incredible thing; a cold as much below the cold of freezing water, as that is below the heat of boiling ſpirit of wine.

* Vid. Newton Princip. p. 508. & in Phil. Tranſ. Abr. IV. 2. p. 2, 3. Pitcarn. El. Med. II. 1. § 26.

ter begins to freeze; and thence found the additional dilatations of quickſilver, or of lint-ſeed oil (or of any other fluid that may be uſed in ſuch Thermometers) to be in theſe proportions; though the abſolute or real quantities of heat in theſe bodies do not differ ſo much by far, as they by that miſtaken way of reckoning computed. Thus boiling water indeed raiſes the mercury in the Thermometer to *gr.* 212, that is 180 diviſions above *gr.* 32, the heat of freezing water, while the heat of the human body is but about *gr.* 97 or 98, only 65 or 66 degrees above the freezing point. And therefore the dilatation acquired from that point by the heat of boiling water, to the dilatation acquired by the heat of our body, is in the proportion of 180 to 66, or 2'73 to 1; which is ſomething leſs than triple. But the real heats of theſe things are ſtill leſs differing from one another. Suppoſe, for example, Fahrenheit's ice and ſpirit of nitre mixture of—*gr.* 40 had been at the loweſt boundary of heat, (though there is great reaſon to think that to be ſtill much lower), and then we ſhall find that the heat of boiling water ſhould be to that of our bodies only as 212+40 to 98+40, or 252 to 138, or 1'83 to 1: that is, not ſo much as double. Who, before ſuch a particular examination, would have thought the burning deſtroying heat of boiling water not to be altogether twice ſo great as the ſoft temperate heat of a man in health? And upon the former reckoning of red-hot iron, its heat ſhould be to the heat of our ſkin inſtead of 10 or 12 to 1, only as about 700 to 138, or 5 to 1. So that by thus bringing down the limits of heat at leaſt 72 degrees below the ordinary reckoning, we find its various characters as *froſt, temperate, warm, tapid, boiling water, red hot iron*, &c. whatever their arithmetical diſtances come out to be, are not ſo different in their real proportions to one another, as is commonly ſuppoſed. Though, on the other hand, I would not affirm them to differ ſo little as Mr. Amontons* reckoned. In whoſe ſcheme the air ſhould entirely loſe its elaſticity, when it is perfectly devoid of heat (of which however I think we are not well aſſured) and then in ſuch a total abſence of heat, were it poſſible to procure it, reducing his numbers to ours, the mercury in Fahrenheit's Thermometer ſhould fall 431 diviſions below the cold of freezing water, or to —*gr* 400 *p. q.* a degree of cold as much below the heat of the human body, as that is below the burning heat of oil or mercury boiling. But as we are not quite certain about this loweſt degree of heat, we muſt, as I juſt now ſaid, content ourſelves to obſerve the arithmetical differences of the various degrees of heat in bodies, though we ſhould not be able to determine their real or abſolute proportions, until we ſhall acquire a farther inſight into the nature of things than we have at preſent. It is good to know the narrow limits of our faculties, and modeſtly to confeſs the bounds of our knowledge: ever keeping in mind that excellent axiom of the Lord Verulam[*], “ Homo naturæ miniſter & interpres tantum facit & intelligit quantum de ordine naturæ *opere* vel *mente* obſervaverit: nec amplius ſcit aut poteſt.” It muſt be by this road of experience and obſervation, pointed out to us by the great Lord Chancellor, if ever we arrive at a higher degree of knowledge than what we at preſent enjoy.

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* Hiſt. Acad. Scienc. 1702. p. 8. Mem. 1703, p. 63, 64, 238.

II. *Of the Heat of the Air.*

12. In our ſearch[†] after the loweſt degree of heat, though we could not arrive at it, we found however in ſome ſeaſons and parts of the world exceſſively great, and as it were monſtrous degrees of cold; and which the inhabitants of the earth can ſcarcely bear; they are intolerable both to animals and vegetables. The quarters of the earth, within the polar circles, were thought by the ancients to be uninhabited by reaſon of the exceſs of cold prevailing there, ανοικητοι δια ψυχος[‡].

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[*] Nov. Orig. I. 1.

[†] § 6——10.

[‡] Diog. Laert. VII. 156.

Frigus iners illic habitat, pallorque, tremorque, ac jejuna fames.

"Quia," (ſays Macrobius*), "torpor ille glacialis "nec animali nec frugi vitam miniſtrat."

Pigris ubi nulla campis
Arbor æſtiva recreatur aura †.

And indeed theſe bleak wild parts of the world, though not quite waſte, are but thinly peopled. And no wonder. There, and in other diſadvantageouſly ſituated places, where ſuch intolerable colds chiefly rage, men muſt ſhelter themſelves in clothes and houſes. The beaſts of theſe countries are many of them provided by nature with a thick warm covering; or muſt retire the beſt way they can from the killing colds of winter, which however often overtake them. The very vegetables themſelves would die were they not commonly in theſe ſeaſons buried under ſnow to ſuch great depths, as the extreme ſeverity of the cold cannot reach. In bitter black froſts we often experience the fatal effects of the want of this ſhelter and protection, even in the humbler tribe of vegetables. And though there be plenty of ſnow, the trees and higher ſhrubs, that are not covered by it, are great ſufferers in the extraordinary ſevere winters, even in the more happy and otherwiſe more temperate countries. As has been often ſeen, and in our own days was experienced through all Europe in the years $170\frac{8}{9}$, $172\frac{8}{9}$, *&c.* *; when the Thermometers pointed theſe low degrees of heat we had formerly occaſion to mention.

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* In Somn. Scip. II. 5. p. m. 110.

† Horat. Carm. I. 22.

13. But theſe and ſuch like exceſſive colds are in theſe parts of the world juſtly looked on as a ſort of irregularities, and out of the ordinary courſe of things. In theſe temperate climates of ours the mercury ſeldom falls under *gr.* 16. And even then it is extraordinary cold. And indeed we are apt to reckon it very cold at *gr.* 24. By the very conſtruction of the Thermometer it is ſlight froſt at *gr.* 32: and continues coldiſh to *gr.* 40, and a little above it.

14. The middle temperature of our atmoſphere in theſe countries is about *gr.* 48, when we cannot call the weather either hot or cold; and is, as it were, a medium of all the ſeaſons, coinciding pretty nearly with the middle vernal and autumnal heat, as obſerved in England by Sir Iſaac Newton, and Dr. Hales, and by Mr. Crucquius in Holland. The French raiſe this middle temperature a little higher. They reckon it equal to the heat of the

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* Phil. Tranſ. Abr. IV. 2. p. 120, &c. Mem. Ac. Sc. 1710, p. 186. Hales Veg. Stat. p. 74, &c.

cave of their Royal Obſervatory: and this, both by obſervation and a calcul, comparing it with Mr. Amontons's Thermometer, I find to be at *gr.* 53 in Roemer's. And indeed this middle temperature of the air we may eaſily conceive to be very different in different countries. It is to be reckoned as a relative ſort of thing, varying according to the various climates and conſtitutions of bodies in them, which by uſe come to be fitted to different temperatures. In the cold countries the air will be found agreeable enough to the inhabitants while it is betwixt *gr.* 40 and 50. In theſe middle climates in our neighbourhood, and not too far ſouth of us, we are beſt pleaſed with the heat of the air from a little under *gr.* 50 to *gr.* 60; while in the hot countries they have their air generally, and can well bear it, ſomething both above and below *gr.* 70.

15. When the heats in the reſpective climates are either much higher or much lower than theſe, both men and beaſts are fain to provide againſt them the beſt way they can. In the cold they ſhelter themſelves in houſes, dens, holes of the earth, *&c.*; and the human kind beſide that provide for themſelves clothes and fires, to protect them from the ſeverity of the weather. In the heats we lay aſide our clothes, or take to the lighteſt ſort; we ſhun the mid-day air, and retire to the cooleſt places; for in the day-time it is generally warmer

in the open air than in houſes. And ſome of the bird kind chuſe to ſhift their country in the different ſeaſons of the year, finding always by this means a temperate enough climate for their conſtitution.

16. But the ſame degree of heat is not equally fitted to all animals. Some kinds naturally chuſe a cold country, while others indulge themſelves in a warmer climate, and find that better fitted to their conſtitutions. And ſo too, though with Dr. Hales * we ſhould deem the moſt genial heat for the generality of plants to be from *gr.* 53 to *gr.* 69, the various claſſes of vegetables have their various temperatures of the air in which they can thrive beſt. Every body knows that ſome plants can live only in the cold countries, being burnt up if tranſplanted to a more ſouthern ſoil; while the natives of the warm countries are chilled to death with us, if not very carefully and artfully protected from the injuries of our ſeverer weather. This piece of culture, propoſed long ſince by the great Lord Verulam †, has been, in our time, carried to a ſurpriſing height. With the help of Thermometers we can ſo adjuſt our ſtoves, green-houſes, and hot-beds, as to imitate the temperature of any

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* Veg. Stat. p. 59.

† Nov. Orig. II. 35. p. 254. 50. p. 348. Nat. Hiſt. 401, 405, 412, 856.

climate we pleaſe; and to ſupport and keep in ſtrong life and vigour the plants which nature has given only to the warmer countries. According to the obſervations of the London gardeners, as marked on Mr. Fowler's Thermometer reduced to ours, I find the kindly degrees of heat for ſome of the moſt curious foreign plants to be theſe. For the myrtles *gr.* 44; oranges *gr.* 47; ficoides *gr.* 50; Indian fig *gr.* 53½; aloe *gr.* 57; cereus *gr.* 60; euphorbium *gr.* 63; piamento *gr.* 66; ananas *gr.* 70; melon thiſtle *gr.* 73.

Not as if theſe plants could bear only ſuch and ſuch preciſe points of heat. They, like other vegetables and animals, have a conſiderable latitude that way; and can ſuffer the air both colder and warmer than theſe degrees, which, however, are reckoned moſt adapted to their nature. Though I ſuſpect ſome of them are marked too low, being far under the ordinary and moſt kindly heats in thoſe countries where ſuch plants naturally grow. But there indeed they have a freer and opener air; where conſequently they can bear greater heats than in our ſmothered and pent up ſtoves and green-houſes.

17. Even we in this country do not reckon the air warm till it arrives at about *gr.* 64. It is to us very warm and ſultry at *gr.* 80. And Dr. Boerhaave[*] thought it naturally ſcarce ever exceeded

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[*] Chem. I. 156. p. 192, 207, 213, 274, 278, 553.

gr. 80 or 90 at moſt, reckoning ſuch hot air would ſoon be deſtructive of the life of animals. But ſometimes, even in theſe temperate climates, the weather has been found much hotter. I find obſervations recorded of the weather at Utrecht, Paris, Padua, *&c.* having ſometimes been obſerved ſo hot, as that the mercury in our Thermometer would have been raiſed to above 90 degrees. In Pennſylvania, no intemperate climate neither, the heat in ſummer 1732 came once to be *gr.* 96 or 97 *. And Mr. de Reaumur † promiſes us obſervations of heats that people were obliged to ſuffer, though they reached to *gr.* 38 in his Thermometer, or near *gr.* 104 in ours. Even in the more northernly countries, where the ſun has leſs influence, we can by art contrive a method of giving undeniable evidence of animals being able to bear a ſtill greater warmneſs of air. In the outer bagnio at Edinburgh the heat uſed to raiſe our Thermometer to *gr.* 90, where, though at your firſt entry it ſeemed indeed a great deal too hot, and ſomething diſagreeable, yet ſoon it became ſo very tolerable, that one might without any uneaſineſs have continued in it as long as he had thought fit. Yea, one could ſtay ſome hours in the inner bagnio, though the heat there uſed to be about *gr.* 100.

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* Act Berolin. Cont. IV. p. 131.

† Mem. Acad. des Sc. 1736, p. 486, 489.

18. From all this we ſee how finely every thing has been contrived and adjuſted by the great and wiſe Author of nature, and what a vaſt latitude of the heat and cold of the air the animal body, eſpecially that of man, can bear, from the ſultry burning heats, as hot or hotter than his blood, down to the chilling colds a good deal below that of freezing water; though indeed it is about ſome intermediate diſtance betwixt them, where the heat is moſt agreeable, or beſt tempered to the bodies of animals and vegetables.

19. And it was only the middle climates or temperate zones that many of the ancients imagined were habitable by us mortals.

Mortalibus ægris
Munere conceſſæ divûm [*].

"In his," ſays Macrobius [†], "tantum vitales auras natura dedit incolis carpere." And accordingly much was ſaid long ago of the intolerable heat of the torrid zone, as by that means uninhabitable

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[*] Virgil Georg. I. 237.

[†] In Somn. Scip. II. 5. p. m. 110.

habitable by men, *ανοικητος ὑπο καυματων*[*]: whence Ovid[†], ſpeaking of the zones of the earth, ſays,

Quarum quæ media eſt non eſt habitabilis æſtu.

Virgil[‡] ſays the ſame thing in a more pompous manner.

Quarum una coruſco
Semper ſole rubens, & torrida ſemper ab igni.

And Horace[||], with his ordinary poetical imagery, ſuppoſes himſelf placed,

Sub curru nimium propinqui
Solis, in terra domibus negata.

But Tibullus[**] expreſſes the poſition of this part of the earth, and its conſequences, more fully than any of them, in theſe lines:

Media eſt Phœbi ſemper ſubjecta calori,
Non ergo preſſo tellus conſurgit aratro:
Nec frugem ſegetes præbent, nec pabula terræ.
Non illic colit arva Deus, Bacchuſve, Cereſve,
Nulla nec exuſtas habitant animalia parteis.

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[*] Stoics in Diog. Laërt. VII. 156.
[†] Metamorph. I. 49.
[‡] Georg. I. 234.
[||] Carm. I. 22.
[**] Eleg. IV. 1. in Meſſal.

But this opinion was, I humbly think, among their vulgar errors, received chiefly by the people, and taken up by the poets as a beautiful fiction, which we cannot well ſuppoſe they really believed. The wiſer ſort, though they ſometimes ſpoke * like other folks, knew better things. Their hiſtorians and geographers have recorded tolerable good accounts of ſome of the middle parts of the earth within the tropics: Æthiopia, Arabia Felix, the ſea-coaſts of India, the iſle of Taprobana, Ophir, Tarſhiſh, &c. were heard of even by the common people, all well inhabited, though lying within the torrid zone: and the travels of the ancients, as well as ours, ſhewed theſe countries not to be diſagreeable or inconvenient for human life. Nay, the heats there are by no means ſo exceſſive as we are all, before trial, ready to imagine and expect. The Jeſuit miſſionaries to the Eaſt Indies † took particular notice of this: and their capacity of obſerving ſuch things, and their fidelity in relating them, no body will call in queſtion And lately the curious Mr. Coſſigny's thermometrical obſervations ‡ ſhew, that the ordinary heats, even under

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* See Cicer. Somn. Scip. 6. Macrob. in Somn. Scip. II. 5, 6, 7. Diog. Laërt. VII. 156.

† See Du Hamel Hiſt. Ac. Sc. p. 272, 273. Hiſt. Acad. Sc. 1666—1698, II. p. 111, 112. Mem. Acad. Sc. 1666—1698. VII. p. 835.

‡ Mem. Acad. Sc. 1733, p. 580, &c. 1734, p. 759, &c.

the line, and near it, are not greater than what have been in ſome extraordinary ſeaſons obſerved at Paris, and other places, that every body knows to be temperate enough.

20. It is true all theſe obſervations were made in iſlands, and on or near the ſea-coaſts, which we may preſume to be ſomewhat cooler than the inland countries*. But ſtill we are well aſſured, that even theſe can eaſily enough be borne with. And all theſe parts of the world have that advantage of the weather, there being much more uniform than with us. Their air ſuffers much ſmaller changes both in the incumbent weight of the atmoſphere preſſing it, and in the degrees of heat wherewith it is warmed. Dr Halley † tells us, that he, as well as others ‡ who have ſojourned within the tropics, found very little variation in the height of the mercury in the Barometer, which we know to be 2 or 3 inches in the more northern regions: and we are all but too ſenſible, and we had but juſt now occaſion to take notice of the vaſt changes of the temperature of the air in this quarter of the world, which through the whole year is commonly found ſo equable in the torrid zone. At Siam, within the lat. 15 deg. from the greateſt winter cold to their

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* See Mem. Acad. Sc. 1666—1698, VII. p. 837.

† Phil. Tranſ. Abr. II. p. 18, 20, 22.

‡ Vid. Mem. Acad. Sc. 1666—1698, VII. p. 840.

higheſt ſummer heat the Jeſuits experienced a difference only of 26 degrees in Hubin's Thermometer *, the extent of which I believe differs not much from the like number in ours. † In Batavia, though a very hot place, and in *gr.* 6 ſouth lat. the exceſs of heat above the greateſt cold was ſtill ſomething leſs: and in Malacca ‡, but 2 deg. of latitude from the equator, the weather is very temperate, and ſo equable, that for 7 months together the ſpirit in the Thermometer was never under *gr.* 60, nor above *gr.* 70. ‖ In the iſle of Bourbon, in the latitude about 22 degrees the difference between the higheſt and loweſt afternoon heat in a twelvemonth's time was but 8 diviſions in De Reaumur's Thermometer, which makes up ſcarcely 15 in ours: and the ordinary difference of the night and day heat in theſe climates was found a ſmall matter compared to what we have it commonly with us **. And as the torrid zone excels our countries hereabout in a more equable degree of heat and weight of the atmoſphere, ſo we are more happily appointed that way than in Sweden and Lapland, and theſe other northern regions *. From whence it would ſeem a general rule, that, *cæteris paribus*, the greatneſs of the viciſſitudes of the air increaſes with the latitudes of the places as you go from the equator.

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* Du Hamel Hiſt. Ac. Sc. p. 272.

† Ibid. p. 273.

‡ Ibid.

‖ Mem. Ac. Sc. 1734, p. 762.

** Ib. 1666—1698, VII. p. 837, 1733, p. 537.

III. *Of the comparative Heats of the Sun, Earth, Planets and Comets.*

21. We have hitherto conſidered the air, though influenced by the ſun, as ſhaded from its immediate burning operation. For in the open air, expoſed to its direct beams ſhining with greateſt advantage, the heat is found much higher than any thing we have hitherto been ſpeaking of. And an examination of this will naturally lead us into the conſideration of the heat of the ſun, the inexhauſtible magazine of light and fire, the great furnace of our ſyſtem; and to conſider its influence on bodies placed at different diſtances from it.

Sir Iſaac Newton †, comparing the ſun's influences on the earth and Mercury, which by its vicinity to the ſun ſhould have ſeven times more heat from him than we, reckons the ſolar heat in that planet ſufficient to boil our water, and quickly carry it all off in vapour; having found by a Thermometer that water was boiled by a heat ſeven times greater than the heat of the ſummer ſun; which then in this reckoning ſhould be only at *gr.* $(32+\frac{180}{7}=)$ 58. But this expreſſion of *the heat of the ſummer's ſun* muſt be greatly modified to be conſiſtent with the true ſtate of things. He muſt not only have meant it ſolely of the degrees above freezing water, (as was then the faſhion of thinking and ſpeaking), but alſo inſtead of *ſummer ſun*, he muſt really have underſtood *the ordinary ſhaded air in ſummer*, or *the ſummer heat of the air*, as Dr. Pitcairn * (who I ſuppoſe had his obſervations of this matter from Sir Iſaac) expreſſes it. Though I muſt confeſs that this could be but very improperly compared to the ſolar heat at Mercury; except we ſuppoſe that he likewiſe meant the heat in the ſhade at Mercury too. And farther, it will be reckoned an incongruous way of judging of the heat of the ſun, by taking our meaſures from its impaired action on the air ſcreened from its direct rays.

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* See Phil. Tranſ. Ab. II. p. 22. Maupert. Fig. de la Terre, p. 72.

† Princip. III. Prop. 8. Cor. 4. p. 406.

22. The direct ſummer mid-day heat is vaſtly greater than ſuch a low allowance; it having been found, even in theſe northern climates, able to dilate the fluid in the Thermometer much more than $\frac{1}{7}$ of the dilatation cauſed by boiling water. Dr. Boerhaave is the loweſt in his reckoning of any that pretended to take an eſtimate of it, ſaying †,

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* Elem. Med. II. 1. § 26.

† Chem. I. p. 213.

that the greateſt heat ever produced by the direct action of the ſun in our air, or other bodies heated by it, ſeldom reaches to *gr.* 84. And at another time *, though he raiſes it a little higher, ſeeming to allow that the great dog-day heat may come to *gr.* 90, he thinks it can ſcarce ever go beyond theſe bounds, or arrive at the heat of the human body. But Sir Iſaac himſelf, in another place of his book †, expreſsly tells us, that he found the heat of boiling water only about three times greater than the heat communicated to dry earth by the ſummer ſun. And ſo, too, Dr. Muſſchenbroek ‡ reckoned it probable that the heat of boiling water is three times greater than the greateſt heat communicated to bodies by the ſummer ſun in his country; by theſe reckonings this heat amounting to *gr.* $(32+\frac{180}{3}=)$ 92. Nay, even in Italy, Borelli ‖ and Malpighi § found this heat of the ſun in mid-ſummer only equal to the heat of the *viſcera* of hot animals, I ſuppoſe about *gr.* 102; not a deal higher than what Newton and Muſſchenbroek reckoned.

23. All this may, I believe, commonly be juſt and true enough. But it is not ſo univerſal, either

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* Chem. I. p. 156.

† Princip. p. 508.

‡ Tent. Exp. Ac. Cim. Add. II. p. 22.

‖ De Mot. Anim. II. Prop. 96, 221.

§ Op. Poſt. p. 30.

as to times or places, as one would be ready to expect from ſuch ways of ſpeaking The Lord Verulam * long ago took notice, that the higheſt degree of animal heat ſcarce ever attained to the heat of the ſun's rays in the ſcorching climates and ſeaſons. And even in this cold country at ſometimes I have experienced greater heats of the ſun; and ſo might any body elſe that would attend to it have done. Many have had occaſion to obſerve how vaſtly metals are heated in the ſun. Sir Iſaac Newton † takes particular notice of this; I have in this place in the latitude of 56 deg. 20 min. ſeen a piece of iron heated ſo much that I could not hold it in my hand above a few ſeconds of time; and therefore it was much hotter than my blood. And I have oftener than once ſeen dry earth heated to above *gr.* 120. And Dr. Hales ‡ found a very hot ſunſhine heat *an.* 1727 ſtill greater, *viz.* at about *gr.* 140. And Dr. Muſſchenbroek § in his later trials once experienced it ſo high as *gr.* 150. So that reckoning from the freezing point at *gr.* 32 in the common way, theſe ſtrong ſun-ſhine heats were much greater than $\frac{1}{3}$, or even the half of the heat of boiling water. And in the more ſouthern countries it has been found ſtill greater at ſome particular times. Much has been ſaid of the ſcorching and intolerable heat of the ſun in the torrid zone *. We have many ſtrange ſtories of extraordinary ſummer heats, as great tracts of land, houſes, *&c.* ſet on fire, ſtones heated ſo as to melt lead, *&c.* Theſe indeed ſeem extravagant. But the German annals† preſerve the memory of an exceſſively hot ſummer *an.* 1230, when they roaſted their eggs in the ſand heated by the ſun. And I have been told that in Egypt, by no means the hotteſt country in the world, they can often on the tops of their houſes roaſt their eggs at the ſun. And to harden the white of an egg I find the heat of about *gr.* 156 to be neceſſary. In the year 1705 the ſummer was very warm ‡. At Montpelier one day the ſun was ſo very hot as to raiſe the quickſilver in M. Amontons's Thermometer to the mark of boiling water itſelf, which is our *gr.* 212.

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* Nov. Orig. II. 192.

† Princip. p. 420.

‡ Veg. Stat. p. 59.

§ Eſſ. Phyſ. § 974.

24. But after all, theſe great heats will be reckoned irregularities, and out of the ordinary courſe of the ſeaſons; and what may be produced by the ſun only in certain times and circumſtances. For in our temperate climates the moſt common and ordinary noontide heat of the ſummer ſun may be found to raiſe the quickſilver in the Thermometer but about 60 or 70 diviſions above the freezing

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* See Boyle Hiſt. of the Air. Abr. 3. p. 55.

† See Lipſii Epiſt. ad Belg. II. 91.

‡ Mem. Sc. 1706. p. 15.

point at *gr.* 32; and the afternoon heat in the ſhade about the half of that. So the ordinary ſummer ſun heat with us does nearly coincide with the heat of our body; which was found * to be ſomewhat more than the half of the heat of boiling water, ſuppoſing Fahrenheit's cold mixture to be the very loweſt degree of heat; and ſo too boiling water, inſtead of ſeven times, is but between two and three times hotter than our common ſhaded warm air at midſummer.

25. However, this ſolar heat is ſtill much greater in the warmer countries. And if we pretend to make any compariſon of ſuch things, it is our equatoreal heat we ſhould reckon as the ſun's true heat at our orbit, and what we ſhould collate with its influence on the other planets, or its heat at their reſpective diſtances.

26. But what, on the other ſide, will lower or bring down this ſolar heat very much is this conſideration, that the heat communicated by the ſun to bodies on this earth depends much upon other circumſtances beſide the direct force of its rays. Theſe muſt be modified by our atmoſphere, and variouſly reflected and combined by the action of the ſurface of the earth itſelf, to produce any notable effects of heat. For were it not for theſe ad-

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* See § 11.

ditional circumſtances, I queſtion much if the naked heat of the ſun would to us be very ſenſible. We find all our great heats to be in thoſe places that ly low, and have a great height of atmoſphere above them, and ſurrounded by eminences and riſing grounds. * If you aſcend on high to the tops of the very elevated mountains, you are chilled with cold, and you find everlaſting ſnows, that after thouſands of ſummers have ſcarce ever been thawed, though every day expoſed to the direct rays of the ſun; which in ſome countries are darted perpendicularly on them. We hear that the French virtuoſi, who by order of the king went to America to make their obſervations on the figure of the earth, *&c.* found on the very high hills there juſt under the line ſuch exceſſive colds, that it is ſuſpected they were as intenſe as what were obſerved in Lapland itſelf. So neceſſary it ſeems is a long and direct paſſage through all or the greateſt part of the depth of our atmoſphere, or the aſſiſtance of its preſſure, or the reflections of rays from the earth's own ſurface, to invigorate theſe rays, and to give them ſtrength for warming terreſtrial bodies. To which too the particular ſulphureous nature of the low parts of the atmoſphere may not a little contribute.

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* See Liv. Hiſt. XXI. 31. Senec. Nat. Qu. IV. 11. Bacon. Nov. Org. II. 12. p. 169. Borell. de Incend. Actu. p. 7, 50. Boyle Exp. on Cold. Abr. I. p. 639, 658. Hiſt. of the Air, Abr. III. p. 53, 54, 55. Boerh. Chem. I. p. 172, 185, 476.

27. So then there are other circumſtances beſide the vicinity to the ſun that may influence and determine its heating virtue. Thus reckoning the force of heat to be *cæteris paribus* as the denſity of its rays, or reciprocally as the ſquares of its diſtances from the central fire, the planet Mercury is ſo much nearer the ſun than the earth, that its heat from him, by calculation of their reſpective diſtances, comes out ſeven times greater than ours *. And ſo would an inhabitant there feel it, if they have the ſurface of Mercury diſpoſed like that of our earth, and an atmoſphere hanging over their heads ſo like ours, as to qualify the ſun's rays in the ſame manner. But if they have little or no atmoſphere (which ſeems to be the caſe of our moon) or an atmoſphere ſo diſpoſed as only to affect the rays of heat in the manner that the upper parts of our atmoſphere do, then it is poſſible the ſolar heat at Mercury may be as tolerable as on our earth. And, on the other hand, though we commonly reckon † that our waters ſhould be frozen up, and we ourſelves be chilled to death in the far-off planets Jupiter and Saturn, they may, for aught we know, have their atmoſpheres and

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* See Newton Princip. III. Prop. 8. Cor. 4. p. 406.

† See Newton, ibid. p. 405.

ſurfaces ſo conſtructed, as to render the ſolar heat comfortable enough even for animals and vegetables of the like nature and paſſions with our own.

28. But the caſe of the comets is vaſtly different from any thing we can propoſe of the other planets. Their orbits are ſo excentrical, that they muſt be very variouſly affected by the ſun in the different parts of their courſe. But yet their atmoſpheres and bodies may be of ſuch a nature as that they ſhall not be heated to ſuch a vaſt and almoſt inconceivable degree as is ſometimes ſuppoſed. In the Newtonian ſyſtem of comets * the diſtance from the ſun of that famous one which appeared *an.* 1680 † in its perihelion was, by calculation, to the mean diſtance of the earth in the ratio of about 6 to 1000: ſo that its heat in that

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* Newton Princip. III. &c. p. 508.

† It is ſuppoſed by many to have been this very comet that appeared *an.* 1106, *an.* 531, *an.* A. C. 44, &c. See Newton Princip. p. 501. Halley Synopſ. of Comet, in Gregor. Aſtron. in Engliſh. p. 901, 902, 903. Whiſton New-Theor. of the earth. p. 187, 191, &c. But M. Caſſini repreſents the comet, or comets, *an* 1680 in a very different manner from theſe authors; nor does he make that great blazing ſtar that alarmed all Europe, to have been ſo very excentrical, or to have come ſo very near the body of the ſun as they do. See Mem. Acad. Sc. 1731, p. 464—468.

part of its period Sir Iſaac determines ſhould have been to our ſummer heat reciprocally as the ſquares of theſe numbers, that is, as 1,000,000 to 36, or 28,000 to 1. And therefore, ſeeing the heat of boiling water is three times greater than what dry earth acquires from the ſummer ſun, and the heat of red-hot iron three or four times greater than the heat of boiling water, he computes the heat of the comet ſhould have been 2000 times greater than the heat of red-hot iron. A prodigious and terrible heat, even with all the abatements we have been propoſing; and which I conceive no body can take on, that is not immenſely denſer and more fixed than any terreſtrial body we know. For, from the example of common water, *&c.* * it would ſeem that all bodies are, according to their various degrees of denſity and fixedneſs, ſuſceptible juſt of ſuch and ſuch particular degrees of heat, beyond which, whatever fire be applied to them, they cannot be raiſed; though by a heat exceeding theſe their parts are all diſſipated, are rendered volatile, and fly off in vapour. And yet I cannot but take notice, that all this calculation is upon the ſuppoſition that the comet was in its own nature like our earth, and involved in an atmoſphere like ours: otherwiſe that method of reaſoning and analogy cannot take place, nor would the ſun's heat affect it in that way.

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* See Boerh. Chem. I. p. 265, 248.

29. But I ſay again, under another view and conſideration, what if the real ſolar heat, both in itſelf and what it can communicate to us and other planetary bodies, (while it is not concentrated by burning glaſſes, or ſtrengthened by other aſſiſtances), be vaſtly leſs than what is commonly reckoned? All the natural heat we meet with here on the earth we are ready to aſcribe to the action of the ſun, which perhaps has but a ſmall ſhare in it, overlooking a ſource of heat, which, though often ſpoke of by the theoriſts of the earth *, is ſeldom conſidered in that advantageous light I would chuſe to take it. We formerly took notice of what a great ſtock of heat all terreſtrial bodies are poſſeſſed, even in the coldeſt winters Every body has felt or heard that the temperature of the air in mines and other places deep under ground is warm, or at leaſt very tolerable. And we know, from the niceſt obſervations, that in the cave of the Obſervatory at Paris, only about ninety feet under ground, the heat keeps the Thermometer at *gr.* 53, and that without any aſſiſtance from the ſun; it being never ſenſibly increaſed by the moſt

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* See Empedocles in Plutarch. de Prim. Frigid. p. m. 507. Des Cartes Princip. Phil. IV. 3. Burnet Theor. of the Earth, III. 6. Woodward. Eſſ. Nat. Hiſt. of the Earth, p. 135—162, 220—225. Nat. Hiſt. &c. Illuſt. &c. Introd. p 136, 140—143, 149—152. Whiſton New Theor. &c. p 78, 231, 334, 447. Gaſſend. Epicur. Philoſoph. I. p. 546, &c.

ſcorching ſeaſons beyond its heat in the moſt ſevere winters that have been felt there. And the ſame conſtant and unalterable degree of heat was obſerved by Mr. Boyle * in a cave cut deep into the earth. † And great and even troubleſome heats are ſaid to be obſerved at greater depths, and increaſing in proportion to theſe depths; though I could wiſh theſe heats had been more regularly meaſured and aſcertained than what I can find they have yet been. So that it would ſeem the body of the earth has a very great proper internal heat, independent on the ſun, and very much beyond what he, without the intervention of our atmoſphere could communicate to it; ſo great as, within 90 feet of its ſurface, to raiſe the Thermometer 93 diviſions above Fahrenheit's cold mixture, or 453 diviſions above what Mr. Amontons reckoned the loweſt degree of heat. This heat of the earth at its ſurface is ſomething leſs, and beyond that its force decreaſes indeed very faſt, ſo as to leave the air at a ſmall height above it a good deal colder; and which we find on the very high hills to be exceſſively cold, and not to be much warmed by the additional heat of the ſun's direct rays, if they be but little altered by the earth's ſurface and atmoſphere.

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* Mem. Hiſt. of the Air, Abr. III. p 54.

† See Boyle Exp. on Cold, Abr. I. p. 641, 700, 701, 702. Boerh Chem. I. p. 479. Woodw. Eſſ. Nat. Hiſt. of the Earth, p. 136.

30. It is not our buſineſs ſo much as to attempt the explaining the cauſe and conſequences of this innate heat. That is the work of thoſe who have fire and fancy enough to build worlds, or theories of worlds. I will only beg leave to obſerve, after the Lord Verulam *, Mr Boyle †, and others, that the various temperatures of the air in different places of the earth, do by no means correſpond to what ſhould be the reſult of their poſition to the ſun. To omit variations that are ſmall and of leſs moment, I ſhall only take notice of the much greater cold in all the ſouthern hemiſphere than in the ſimilarly ſituated places of the northern one; and of the greater colds in the North American countries, than in thoſe of the ſame latitude on the European ſide of the globe. All which argues a copious fund of ſome other more potent cauſe of heat than the regular actions of the ſun; and that that cauſe, inherent in the earth itſelf, is ſtronger in ſome regions of it than in others, though every where conſiderable and of great force.

31. The ingenious Dr. Halley ‡ has a very bold hypotheſis, which, beſide other purpoſes, he employs

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* Seq. Chart. p. 102.

† Exp. on Cold, Abr. I. p. 657—660, 670. Mem. Hiſt. of the Air, Abr. III. p. 52.

‡ Phil. Tranſ. Abr. VI. 2. p. 41.

ploys in accounting for this anomaly of theſe warm and cold quarters of the world. He imagines that once the earth moved round an axis very different from the preſent, and ſo that " the extreme cold " felt in ſome places, as in the north-weſt of Ame-" rica, about Hudſon's Bay, may be occaſioned " by thoſe parts of the world having once been " much more northerly, or nearer the pole than " now they are, whereby there are immenſe quan-" tities of ice yet unthawed in thoſe parts, which " chill the air to that degree, that the ſun's " warmth ſeems hardly to be felt there ;" which, after ſome thouſands of years, have not yet, it ſeems, acquired a ſufficient heat: while by a parity of reaſon we may from the ſame principles infer, that the extraordinary ſoft temperature of ſome other places may be owing to their having originally been under or near the primitive equator, whereby they had acquired a heat which is not yet quite diſſipated.

But, beſides that in ſuch an hypotheſis thoſe places ſhould conſtantly be coming nearer and nearer to a regularity and uniformity with other countries alike in latitude, and other ordinary circumſtances, the heat of bodies is ſo tranſient and fleeting, and the influence of the ſun reaches ſo little way into the body of the earth, that I cannot conceive, even allowing the ſuppoſition of a changed axis, that the various degrees of the original primitive heats of the earth, communicated to it by the ſun, could poſſibly have been ſo permanent and laſting, as to ſhew themſelves ſo effectually at this day: ſo that, as I ſaid before, ſuch a variety of heats muſt rather be owing to an internal cauſe operating more intenſely in ſome regions than in others, and every where much more ſtrongly than any influence the ſun can give them.

32. And if this be the caſe of the reſt of the planets as well as of our earth, their heats, or the heats of bodies on their ſurfaces, will not ſolely nor even chiefly depend on the action of the ſun on them, (as the philoſophical aſtronomers * ſeem by their manner of ſpeaking to ſuppoſe), theſe proper innate heats having a greater ſhare in them than that external addition. Though this, however, may be neceſſary to ſupport and preſerve thoſe entire; ſeeing all warm bodies, that are not endowed with a vital principle of heat, are, of their own accord, liable to a conſtant waſte and decay of this quality.

33. But whatever be in this, from theſe conſiderations, the heat of the ſun, and the heats of bodies approaching near him, will not be found ſo

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* See Newton Princip. p. 405, 406, 508, 509. Gregor. Elem. Aſtron. VI. Prop 2, 3, 4, 5, 6. Whiſt. Præl. Math. p. 327, 328. New Theor. &c. p. 53, 78. Hugen. Coſmotheor. II. p. 694, &c. Derham Phyſico-theol. p. 171—178.

exorbitant as we have been accuſtomed to reckon them *. The proper or innate heats of the planets may be ſuch, that the inner ones are in no hazard of being too much ſcorched, nor the far-off ones of having too ſmall an allowance of the benign influence of the ſun's heat; and that, eſpecially by the help of peculiarly diſpoſed atmoſpheres concurring that way, there may be a great uniformity, in this reſpect, of the whole ſolar ſyſtem. Yea, even the comets themſelves may, for any thing we know, be ſo conſtructed as to ſuffer no great inconvenience by their great excentricities, and very variable diſtances from the central fire.

34. But where am I going? I find myſelf treading on ſlippery and inchanted ground. I fear I may be thought juſt now to have got too far into the bewitching and airy regions of fancy and conjecture, where I would not chuſe to dwell. However, at preſent having a delightful proſpect in view, I would fain be indulged in one queſtion before I quit this place. What if, abſtracting from the other ſolar influences of light, gravitation, *&c* the great force and virtue of its additional heat on the bodies of planets (which is wiſely contrived to be very variable and ſhifting) is to produce frequent changes on the otherwiſe equable heats of their ſurfaces, for very great and noble purpoſes

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* See Newton, ibid. p. 508. Whiſton, ibid.

in the life and ſupport of their inhabitants? as at leaſt we know to be the ſtate of our globe, which, by the viciſſitudes of night and day, and winter and ſummer, and ſpring and autumn, beſide other ends thereby accompliſhed, preſerves in action and life both vegetables and animals, all which would ſtagnate and die in a ſtate of heat always perfectly the ſame: for all natural organic bodies, from the loweſt vegetable to thoſe of the higheſt animal life, have, by the viciſſitudes of heat and cold they neceſſarily undergo, a conſtant oſcillation, a perpetual alternate contraction and expanſion of all their ſolid and fluid particles.

35. So then the ſun, though it be not the ſole or chief fountain, is, as it were, the great regulator of motion, heat, and life to the inhabitants of this ſyſtem; and on this account the benefits, to us men, of this great mover, this awakener and enlivener of nature, are highly to be prized; which, however, were much miſtaken by the ancient heathen nations, and ſtill continue to be ſo amongſt many in the world, who having loſt the true religion and philoſophy of things, that were given to the firſt men, and ſo ſeeing things but by halves, and dazzled by the ſplendour of the ſun's light and power, and ſenſible influences*, imagined, that for their life and all the goods thereof they depended

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* Wiſdom of Solomon, xiii. 1—7.

on that great luminary, and ſo worſhipped the creature inſtead of the Creator, the work inſtead of him that made it, the knowledge of whom they had utterly loſt. And this was never to be recovered without a new revelation, a new communication of himſelf from God to man, or a more accurate ſurvey and knowledge of the ſyſtem of nature than what mankind could of themſelves ſoon or eaſily arrive at, by which the ſeveral connections, and ſubordinate dependencies of its parts one with another, and the great and univerſal dependence of the whole on a wiſe and omnipotent Maker and Ruler might be found out.—An inquiry indeed of the greateſt concern to us men, and in which there have been very great ſteps made in theſe latter times, that do very much confirm and illuſtrate the revelation given to Abraham for the conſervation of the true religion, and continued to his poſterity: which was afterwards much farther extended by the laſt grand revelation concerning the great Creator, the Lord of heaven and earth, that made the ſun, earth, and ſtars, and governs them all with infinite wiſdom and power.

IV. *Of the degrees of Heat in Animals.*

36. Thus much for the celeſtial and planetary heats. But now let us come nearer home, and

conſider the various degrees of heat in the living inhabitants of this our earth, which we can ſee and feel, and examine by our ſenſes: a very fruitful inquiry, and of greater immediate ſatisfaction and uſe to us, being what we are more particularly intereſted in.

The heat of animals is extremely various, both according to the variety of their kinds and the variety of the ſeaſons. Zoologiſts have juſtly enough divided them into *hot* and *cold*, that is to ſay with reſpect to our ſenſes. Thoſe that are any thing near our own temperature we call *hot*, while all the others are reckoned *cold*, whoſe heat is much below ours, and conſequently affects our feeling with the ſenſe of cold, though, according to what trials I have had opportunity of making, they are all, by their *vis vitæ*, kept ſomething warmer than the medium in which they live.

37. The gradations of life from the warmeſt animals down to perfectly inert matter are very ſlow, and by very ſmall and almoſt inſenſible degrees, ſeeing, as it is not eaſy to determine the loweſt ſtate of vegetation, ſo too the limits between the higheſt vegetables and the loweſt animals are with difficulty to be diſtinguiſhed. Now all bodies partake of the heat of the medium ſurrounding them, as air, water, or any thing elſe. Nor can I find by the moſt careful experiments purpoſely made, that vegetables, any more than the moſt inert

and lifeleſs maſſes of matter, are endowed with any *vital heat* beyond the temperature of the ambient air, to be diſtinguiſhed by the niceſt Thermometers. As the air is a thin body, eaſily heated and cooled, ſo indeed when it is in a cooling ſtate, as chiefly in the evenings and night ſeaſon, it is colder than the plants in it. But on the other hand, for the ſame reaſon, through moſt part of the day it appears, and really becomes warm ſooner and more ſenſibly than they can do. But, when all are kept in an equable ſtate, there is no difference of heat to be obſerved, the plants always correſponding exactly to the temperature of the ambient medium. “In vegetabilibus & plantis nullus reperitur “caloris gradus ad tactum, neque in lachrymis ipſorum, neque in medullis recenter apertis*.”

38. Nay the animals of low life have but very little additional heat beyond that of the air or water. The inſect tribe greatly diſappoint our expectations, ſeeming to be the moſt tender and delicate of all animals, and yet being thoſe that can unhurt bear the greateſt colds. They are preſerved in the coldeſt ſeaſons with little other ſhelter than the leaves and ragged barks of ſhrubs and trees, holes of the walls, or a very ſlight cover of earth; and ſome expoſe themſelves quite naked and open. †Even in the bitter winters of $170\frac{8}{9}$ and $172\frac{8}{9}$ the

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* Bacon Nov. Orig. II. 12.

† Boerh. Chem. I. p. 287; 415.

inſect eggs, nymphas, and aurelias, outlived the colds that were intolerable to the livelier animals: and we know how vaſtly low the fluid then ſubſided in the Thermometers. Mr. de Reaumur [*] found ſome very young ſlender caterpillars that were able to bear a cold under *gr.* 4: and, what is ſtill more, the French mathematicians in their Lapland journey in the autumn were peſtered with innumerable ſwarms of flies of various kinds [†], whoſe eggs and aurelias muſt have wintered under ſtill greater colds than this: and I find that caterpillars have but a ſmall degree of heat, about a diviſion or two above the air they live in.

39. The whole inſect tribe is commonly brought under the claſs of cold animals [‡]: on which account I cannot but take particular notice of a very ſingular exception in the heat of bees, a very remarkable branch of that *genus* of living creatures. As theſe, according to the curious obſervations of naturaliſts, have ſomething very peculiar in their economy, fabric, and generation, ſo I have ob-

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[*] Mem. Ac. Sc. 1734, p. 257.

[†] Maupert. Fig. de la Terre, p. 12. See too Boyle Exp. of Cold, Abr. I. p. 661.

[‡] Vid. Bacon. Nov. Org. II. § 11. p. 167. § 12. p. 186. § 13. p. 192. Nat. Hiſt. 73. Leiſter de Bucc. p. 245.

ſerved that they have a very ſingular conſtitution in the heat of their bodies. This I have experienced frequently; and I find that the heat of a ſwarm of bees raiſes a Thermometer buried amongſt them above *gr.* 97; a degree of heat nothing inferior to our own.

40. The other animals of low life, agreeable to what I obſerved of the ordinary inſects, have but very little additional heat above that of their ſupporting medium. I could ſcarcely diſtinguiſh any in muſcles and oyſters. It was very little in the branchial fiſhes. In flounders, whitings, codfiſh, and haddocks, the heat was found ſcarce a degree more than the ſalt water they were ſwimming in, even when that was ſo low as *gr.* 41. Nor are the red fiſhes much warmer than they. Some trouts, whoſe heat I examined, were but at *gr.* 62, when the river water they had been ſwimming in was at *gr.* 61*. Fiſh can live in water any thing warmer than freezing, that is, any thing above *gr.* 32.

41. The reſult of ſeveral trials made ſnails about 2 degrees warmer than the air. Frogs and

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* And lately at Paris I found the heat of a carp ſcarcely to exceed *gr.* 54, the heat of the water wherein I examined it. The heat of an eel came out in the ſame way.

land tortoiſes ſeemed to have the principle of heat ſomething ſtronger, *viz.* about 5 degrees higher than the air they breathed in. Which I ſuppoſe to be the caſe of the ordinary breathing animals, who indeed have lungs (but theſe lungs of a wide veſicular form) and have their blood not a great deal warmer than the branchial fiſhes. Such I take to be the ſea tortoiſes, toads, vipers, and all the ſerpent kind, who all have their lungs of the ſame fabric, and the ſame cold conſtitution of blood. But moſt of this ſort of animals are not able to bear very great colds; they in the ſevere ſeaſons retiring into lurking places, where they are tolerably well ſheltered; often perhaps at about the middle temperature of *gr.* 48 or ſo. Their *vis vitæ* is indeed in thoſe ſeaſons very low[*], and their waſte of ſubſtance exceedingly ſmall[†]. And I ſuppoſe the ſame may be ſaid of the ſwallows, and other ſleeping birds and beaſts, who, though naturally warm animals, much warmer than thoſe already mentioned, are however in their inert ſtate probably much colder than in their active and lively ſeaſons.

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[*] See Harv. de Mot. Cord. &c. IV. p. 28. XVII. p. 65. Leiſter de Cochl. p. 164. de Bucc. p. 246, 251.

[†] See Phil. Tranſ. Abr. II. p. 825. Leiſter de Cochl. p. 163, 165.

42. For the heat of hot animals is not uniformly the ſame in all animals, and at all times. That admits of a very great latitude; it varies according to their various kinds and the circumſtances of each. The ſurface of their bodies are, like other things, much affected by the heat or cold of the ambient medium, and conſequently wrought upon by all the varieties of ſeaſons and climates, if not ſufficiently ſheltered from their influences. When that is, their external and internal heat is nearly the ſame; but ſtill differing ſomething in different animals.

43. Dr. Boerhaave * indeed ſpeaks of the heat of hot animals as in a natural ſtate pretty uniform, or the ſame in all, reckoning it commonly to be ſuch as to raiſe the mercury in the Thermometer to *gr* 92, or at moſt 94. And ſo, too, Dr. Pitcairn's † heat of the human ſkin at his *gr.* 17, falls near our *gr.* 92. Mr. Amontons ‡, by ſeveral trials, found the heat communicated by the human body to his Thermometer to be at dig. 58$\frac{2}{12}$, 58$\frac{5}{12}$, 58$\frac{6}{12}$, 58$\frac{7}{12}$, 58$\frac{0}{12}$. Theſe, by calculation, I find to fall about Fahrenheit's *gr.* 91, 92, and 93. Sir Iſaac

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* Chem. I. p. 192, 207, 213, 414, 415, 526.

† El. Med. II. 1. § 26.

‡ Mem. Ac. Sc. 1703. p. 235, 243.

Newton's* *gr.* 12, which he makes equivalent to the external heat of the human body, and to the heat of a bird hatching eggs, comes to *gr.* 95$\frac{1}{2}$ in ours. Fahrenheit himſelf† places his heat of the human body and blood at *gr.* 96. And at this mark Dr. Muſſchenbroek‡ ſays, the Thermometer ſtands, when it is immerged in the blood flowing from an animal; though in another§ place he ſpeaks of *gr.* 92 or 94, as a pretty high degree of heat for our blood.

44. I have very carefully made numberleſs obſervations on this ſubject of the heat of animals: and from them I muſt be allowed to ſay, that I find all theſe reckonings much too general, and in the main conſiderably too low. I ſuſpect that, at leaſt in many of them, there was not ſufficient time allowed to warm thoroughly the bulbs of their Thermometers; or perhaps at the time of trial the folks hands had not the natural ſheltered heat. The human kind is amongſt the loweſt of hot animals‖: and yet I by the heat of my ſkin every way well ſheltered, at a medium in an infinite

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* Phil. Tranſ. Abr. IV. 2. p. 1, 3.

† Ibid. p. 18, 52.

‡ Eph. Ultraj. 1728, p. 679.

§ Eſſ. de Phyſ. p. 502.

‖ Cæſalp. Qu. Perip. V. 6. p. 132.

number of trials, raiſe the mercury in the Thermometer to about *gr.* 97 or 98. In ſome it goes a little higher, in others it keeps ſomething lower. Urine new made, and that into a veſſel of the ſame temperature with itſelf, is ſcarcely a degree hotter than the ſkin, as I have found by many repeated obſervations. And that heat of the urine we may reckon nearly equal to the heat of the neighbouring *viſcera*. Dr. Hales * found the heat of his ſkin to be 54, and that of urine new made 58 degrees of his Thermometer, which coincide with our *gr.* 99 and 103, if the calcul that was made of the correſpondence of his Thermometer with Fahrenheit's had been perfectly exact.

45. In the mean time, as I juſt now ſaid, the human kind is among the loweſt of hot animals. Ordinary quadrupeds, as dogs, cats, ſheep, oxen, ſwine, *&c.* by the heat of their ſkin raiſe the Thermometer 4 or 5 diviſions higher than we, as to *gr.* 100, 101, 102, and ſome to 103 or a little more.

46. And the breathing or cetaceous fiſhes are as hot as they, as Dr. Boerhaave † very juſtly reckoned; though he aſcribes too ſmall a heat to

* Veg. Stat. p. 59.

† Chem. I. p. 415.

them, and to all other reſpiring animals, when he confined it within the narrow bounds of 92 or 93 degrees. Thoſe who have had occaſion to travel into the Eaſt Indies* tell us, that the blood of the Manati Fiſh, or Sea Cow, was ſenſibly warm to the touch. And Mr. Richer †, a curious enough obſerver of natural things, found the blood of the porpus as warm as the blood of land animals. I myſelf upon trial found the heat of the ſkin of that amphibious animal the ſea calf, to be near *gr.* 102. In the cavity of the *abdomen* it was about a diviſion higher. In all this, as in many other reſpects, agreeing with our ordinary land quadrupeds; which, in the ſtructure and form of their *viſcera*, the breathing fiſhes do very much reſemble.

47. The Lord Chancellor Bacon ‡ gives it as a current opinion that the bird kind are very warm. They are indeed the warmeſt of all animals, warmer ſtill than quadrupeds by 3 or 4 degrees, as I have found by experiments on ducks, geeſe, hens, pigeons, partridges, ſwallows, *&c.* The bulb end of the Thermometer being lodged in their

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[*] See Ent. Apolog. &c. p. 207. Le Comte Mem. de la Chine, II. p. 343.

[†] See Du Ham. Hiſt. Ac. Sc. p. m. 157. Mem. Ac. Sc. 1666—1668, VII. p. 325.

[‡] Nov. Org. II. 13. p. 193.

groin, the mercury was raiſed to *gr.* 103, 104, 105, 106, 107; and in a hen hatching eggs I once found (for it is not always ſo high) the heat at *gr.* 108.

48. It was, I preſume, without ſufficient ground that the juſt now mentioned noble lord * reckoned the external fleſh of animals, when in action, to be hotter than their blood, *&c.* Generally in my experiments both on birds and quadrupeds the *viſcera* were found near a degree hotter than the ſkin; though ſometimes I could perceive but little or no odds. And a Thermometer thruſt into a fiſtulous ulcer running in amongſt the muſcles of the thigh in a man, ſhewed a heat very little greater than that of the ſheltered ſkin.

49. As the circulation is ſo very quick and free, the heat of the blood in the arteries and veins is pretty much the ſame, and but a ſmall matter, it may be a degree or ſo, above the common heat of the bowels, or near two degrees warmer than the ſkin; a much ſmaller difference than what Sir Iſaac Newton † and Dr. Hales ‡ aſſign; according to whoſe reckonings that would be no leſs than 10 or 11 degrees.

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* Nov. Org. II. 13. p. 193.

† Phil. Tranſ. Abr. IV. 2. p. 1, 2.

‡ Veg. Stat. p. 58. Hæmaſt. p. 98.

V. *Of the preternatural Heats of Animals.*

50. Theſe then are the heats of animals as in my experiments they generally appeared to me. But they are not to be held as univerſal. Beſide the varieties in different individuals, the heat even in the ſame animal does not keep always the ſame. For ordinary indeed, and in health, the variations are not great; but by diſeaſes and extraordinary accidents it may be much altered, and brought both above and below the natural ſtandard. I cannot ſay that I have yet had opportunities of making obſervations enough to lay down any thing ſufficient or ſatisfactory concerning the heat of animals in preternatural or morbid ſtates. However, I think I may venture to ſay, that it is not ſo vaſtly great in fevers as ſome are apt to conjecture.

51. The medical writers all take notice of the conſtant and burning heats in ardent fevers. And we are told * of the Emperor Conſtantius and of ſome others, that they were of ſuch a dry hot temperament, as, when heated by a fever, in ſome meaſure to burn the hands of thoſe that touched them. But this burning heat no body then knew how to meaſure, or to preſerve to us any diſtinct intelligible notion of it. The determining and af-

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* See Bacon. Nov. Org. II. 13. p. 192.

fixing the due bounds to ſuch things being reſerved to this preſent age. The ingenious Dr. Hales[*] ſuppoſes the heat of the blood in high fevers to be about *gr.* 85 in his Thermometer, which comes to *gr.* 136½ in ours; a degree of heat which, I believe, no animal ever arrives at, nay, which I reckon no living creature is able to bear. Dr. Boerhaave[†] furniſheth us with ſome curious obſervations of animals very ſoon deſtroyed in air of *gr.* 146, who all died in leſs time than what would have been neceſſary to bring their bodies up to the heat of *gr.* 136. A Thermometer put into one of their mouths a little after its death ſtood at *gr.* 110.

52. Nor, on the other hand, would I ſuppoſe the fever heat ſo low, or that low heat ſo dangerous, as the great Dr. Boerhaave[‡] ſeems to do. He is afraid of its coagulating the *ſerum* of the blood, reckoning that terrible miſchievous work might be effected by a degree of heat not much above *gr.* 100. Whence Dr. Hales[‖], and Dr. Arbuthnot[§], led thereto by Dr. Boerhaave's authority, affirm,

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[*] Veg. Stat. p. 60.

[†] Chem. I. p. 275.

[‡] Aph. 96, 689. Chem. I. p. 343. II. p. 352, 353, 378, 213, 357, 358.

[‖] Hæmaſt. p. 104, 105.

[§] Eſſ. on Air, p. 114, 211.

that the natural heat of the blood of a human creature approaches very near the degree of coagulation. But I have ſeen fevers, and theſe not exceſſively violent neither, wherein I was aſſured, by obſerving the heat of the ſkin, that the blood was 5 or 6 degrees above *gr.* 100, without apprehending any ſuch danger, or experiencing any ſuch fatal effects*. Such a heat, if neglected or wrong managed, may indeed, as Hippocrates† very juſtly obſerves, diſſipate the more thin and watry parts, and ſo gradually thickening the whole maſs of blood, have bad enough effects that way; if it do not bring on a putredinous thinneſs. But directly to coagulate the *ſerum* of blood, or the white of an egg, by ſeveral trials I found a heat requiſite beyond any thing ever we can experience in a living animal. They continue thin and liquid till the Thermometer points at *gr.* 156 or ſo. Which is an arithmetical mean betwixt the ordinary heat of human blood at *gr.* 99 or 100, and that of boiling water at *gr.* 212; exceeding the one juſt as much as it falls ſhort of the other.

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* In the ague I had lately, during the height of the paroxyſm, the heat of my ſkin was *gr.* 106; and ſo that of my blood *gr.* 107 or 108. And farther, what is very remarkable, in the beginning of the fit when I was all ſhivering, and under a great ſenſe of cold, my ſkin was however 2 or 3 degrees warmer than in a natural healthy ſtate.

† IV. De Morb. XXIII. 23, &c.

53. But no animal, I ſay, can bear a heat near this. Beſide the bad effects it would have upon the fluid, I know our nerves are not able to endure ſuch a ſcorching heat. If that be but a few degrees hotter than our blood, it comes to be intolerable. Sir Iſaac Newton * and Mr. Amontons † felt water too hot for their hands ſtirring in it eaſily to bear at *gr.* 108. I ſuſpect indeed the full heat of the water had not been thoroughly communicated to their too bulky Thermometers. I find it becomes ſcalding hot to my hands and feet at *gr.* 112 or 114. And ſo I reckon will moſt people find, who have not their organs too callous. We ſhould not however judge of the delicacy of the body by the ſenſation of the hands, which in many folks by labour and uſe are rendered preternaturally inſenſible. Mr. Amontons's valet could bear water of the heat of *gr.* 122. And I have ſeen folks who could handle things freely I could not well touch. I doubt not but the hotter animals can likewiſe bear a greater heat than we. And, on the other ſide, fiſhes and cold animals may be ſcalded to death with a heat leſs than that of our blood. A perch, one of the moſt lively of the branchial

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* Phil. Tranſ. Abr. IV. 2. p. 2.

† Mem. Acad. Sc. 1703. p. 236, 244.

ſiſhes, died in three minutes in water heated but to *gr.* 96 *.

54. At firſt, men often gave names to things from certain reſemblances, without pretending thereby to underſtand or exhibit their natures. And in this view phyſicians may continue to talk of the το πυρ, the *febris ardens*, and *inflammations*, and freely uſe theſe and other fiery names, if they mean no more by them (as it is likely they did not in the original uſe of them) than a figurative way of expreſſing extraordinary heats of the body, or the heat of the blood carried conſiderably beyond its natural ſtate. But from undeniable experiments we ſee they muſt be much miſtaken, who not confining themſelves to the figurative uſe of theſe names, and led aſtray by vain and ill-grounded theories, talk of real ſcorchings of the body, and actual burnings of the heart and other *viſcera*, of ebullitions and efferveſcences of the blood, of its being kindled and ready to burſt into flame †, and of other ſuch fiery deſcriptions of heats, which no animal, while in life, can be brought into. So that theſe are ways of ſpeaking, which, to prevent miſtakes, ſhould have no place out of poetry; where alone ſuch bold figures, and theſe expreſſed in ſuch a ſtrong manner, without any reſtriction,

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* Muſſchen. Tent. Exp. Ac. Cim. Add. p. 122.

† Willis Pharm. Rat. II. p. 22.

are to be allowed. Thus ſome people by their profeſſion, may be permitted

> Jecur *to burn, and* Cor *to pierce,*
> *As either beſt ſupplies their verſe* [*].

But for others, who from the nature and great conſequence of the ſubject they deal in ſhould be more ſtrictly tied to truth, gravely to talk of the liver burning up the bile, and ſpeaking of the heart as they would of a piece of red-hot iron, burning the hands of thoſe that touch it [†], or boiling up into froth the blood that falls into it [‡]; and others to give out the ſtomach as a kettle boiling our food by its great heat [||]; ſuch aſſertions, I ſay, as theſe, are unwarrantable impoſitions on the ſenſes of mankind, and ſhould be dropt by all ſober thinking men.

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[*] Prior, Alma, p. 87.

[†] Abu. Jaafar Ebn. Tophail. Philoſ. Autodid. p. 64. Ali Rodoan apud Gom. Pereir. Antonian. Marg. p. 326, 770. Columb. de Re Anat. XIV. p. m. 477, 481. Diemerbr. Anat. II. 6.

[‡] Des Cartes de Meth. 5. p. 29. De Hom. p. 8. De Form. Fœt. § 18, 72. De Paſſ. I. 8, 9. Epiſt. I. 52.

[||] Hipp. apud Celſ. Med. Præf. p. 6. Stukely of the Spleen, p. 35.

55. We ſee then that nature ſeldom carries the body or blood of animals to a degree of heat very much beyond its ordinary healthy temperature. Nor indeed are we able to bear any very great exceſs of that kind, either in the body itſelf, or in the medium without us. We can, however, on the other hand, endure greater exceſſes of cold, at leaſt externally affecting us; there being many remarkable inſtances of the ſurface and extremities undergoing very great colds without being abſolutely deſtroyed. We have all, I ſuppoſe, ſome time or other, even here, experienced very ſharp nipping colds. Theſe, however, are nothing, compared to the frightful accounts we have had of the great prevalency of cold on the bodies of men and other animals in the more northern countries. Often are they quite frozen to death; but, at other times, though the cold has gone a great length, they are happily recovered. We have been told * of animals found ſtiff cold in their lurking places, or buried in lumps of ice, that, by the *ſtimulus* of pain or a warmer air, have been again brought to their life and ſenſes. Partial freezings of the noſe, hands, feet, &c. of the human body are frequent. But, what is much more, there are inſtances † of

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* Boyle Exp. on Cold, Abr. I. p. 671, 673. Queſnay Oecon. Anim. p. 25.

† Fabr. Hild. de Gang. &c. XIII. p. 792.

men being ſo frozen by the lump, that when plunged into cold water they contracted a cover of ice over their whole body, which therefore in all its exterior parts muſt have had a cold conſiderably under *gr*. 32. If it came ſo low as *gr*. 25 the blood muſt have been frozen in ſome of its veſſels. However, even in this terrible threatening ſtate, when the whole body ſeemed to run ſuch a riſk of congelation and the ſleep of death, by good management, well underſtood in theſe intemperate countries, people have been recovered to their life again. In ſuch extreme caſes one would think that the internal heat too muſt have been conſiderably impaired; but how low that may be carried without danger or immediate death, we cannot yet determine. In the mean time, the meaſuring the heat of the birds and beaſts that paſs the winter in a ſtate of ſleep and inſenſibility, might give us ſome light into this matter.

Thus much in general of the heat of animals; that ſubject having been more minutely handled in another treatiſe, *De calore animalium*.

What follows is more in the nature of a plan of what is propoſed or ſhould be done on the reſpective heads, than any thing fully executed on them.

Heads to be added.

VI. Of the heats of waters, oils, and ſalts, according to their fluidity and conſiſtency.

VII. Of the melting and ſhining heats of metals and minerals.

VIII. Of the boiling heats of liquid and melted bodies.

IX. Of the heats of flame and fire.

VI. *Of the heats of Waters, Oils, and Salts, according to their fluidity and conſiſtency.*

The conſiſtency or fluidity of moſt bodies are no eſſential or permanent properties in them; but are rather a ſort of circumſtances or accidents manifeſtly depending on the various degrees of heat applied to them. Some of the hardeſt bodies may be reduced to a fluor by fire. Stones, metals, ſalts, *&c.* ſome by leſs, ſome by greater degrees of heat may be melted down into fluids; which only continue ſo as long as the requiſite heat laſts. On the other hand, moſt of thoſe bodies which to us commonly appear in a fluid ſtate, by cold, or the diminution of heat, may be brought to a conſiſtent form. And if there be ſome fluids which have never been ſeen to freeze, this may, with a great ſhew of reaſon, be aſcribed to the difficulty, or perhaps impoſſibility, of getting altogether rid of heat. And for the greater illuſtration of the doctrine of heat, and to get a farther view into the natural hiſtory and philoſophy of bodies, we will here endeavour to ſettle the limits of firmneſs and fluidity in a great many of them, as far as our own or other people's obſervations can carry us: for the greater uniformity reducing them all to Fahrenheit's ſcale; which we will ſuppoſe protracted even much beyond the heats his Thermometer could bear.

It is a bold, but a very juſt expreſſion of Sir Iſaac Newton*, when he calls "*Water* a fluid taſteleſs ſalt; which," adds he, "nature changes by "heat into vapour, and by cold into ice, which is "a hard fuſible brittle ſtone, and this ſtone re-"turns into water by heat." Agreeable to which view of things, Dr. Boerhaave † calls water a kind of glaſs that melts at a heat any thing greater than *gr.* 32; the boundary between water and ice.

All our ordinary liquors are but water with an additional mixture of oils, ſalts, and earth; and have all or moſt of them been obſerved to freeze, ſome at a greater and ſome at a leſs degree of cold under the freezing of common elementary water.

The animal liquors generally loſe their fluidity eaſily enough. Dr. Boerhaave ‡ ſpeaks as if blood would freeze as ſoon as water, *viz.* at *gr.* 32. But on trial it is found to reſiſt the cold ſomething more. The blood of a lamb was not frozen at *gr.* 29; but it was turned into ice at *gr.* 25 *. And ſome of the cold animals preſerve the fluidity of their liquors ſtill more obſtinately. Some inſects, indeed, were killed, and their bodies hardened, at about *gr.* 20 †. But there are others that reſiſted the cold below *gr.* 4; preſerving the life and ſoftneſs of their bodies, and the fluidity of their juices ‡.

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* Opt. p. 349.

† Chem. I. p. 399, 614.

‡ Chem. II. p. 377.

Milk is not ill to harden, freezing I think at about *gr.* 30.

Ordinary *urine* freezes at *gr.* 28, though if it be ſtrongly impregnated with ſalts and oils it may continue fluid in a cold ſomewhat greater.

Fermented liquors do not freeze ſo eaſily as ſimple water; and the ſtronger they are, they can bear the greater cold; and when they are frozen, it is the weakeſt parts that ſuffer firſt, the ſtronger and more ſpirituous often continuing fluid. Small beer and ſmall wines eaſily turn into ice; and the great colds in Múſcovy, Greenland, Hudſon's Bay, *&c.*

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* Reaumur in Mem. Ac. Sc. 1734. p. 260.

† Ibid. p. 259.

‡ Ibid. p. 257, 258.

froze the ſtrongeſt wines, as ſtrong claret, Malaga, Alicant, *&c.* *

Such colds were certainly very great; but thoſe who had occaſion to obſerve them, had not the means of taking any exact eſtimate or determined meaſure of them; which, if an opportunity were offering, we can do now with greater certainty †. Not only the ſtrongeſt wines may be frozen. Brandy itſelf has been converted into ice ‖; the ſtronger part, if there be bulk enough of the liquor, uſually retiring to the center, (as in the freezing of ſtrong wine), and there conſtituting a ſtronger ſpirit of wine ſeparate from the reſt. And ſuch muſt undoubtedly have been a very great cold. Brandy eluded Dr. Muſſchenbroek's ‡ attempt to freeze it with the cold of ſnow and beaten ice. A mixture of tolerably well rectified ſpirit of wine and water in equal parts, which I reckon would make a liquor about the ſtrength of common brandy, was not frozen but with the cold of *gr.* 7. A weaker mixture of 1 part ſpirit to 3 of water, froze at *gr.* 7. But, on the other ſide, a ſtrong mixture, where the ſpirit was double the water, did not freeze till the cold came down to —*gr.* 11 [*].

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* See Boyle Exp. on Cold, Abr. I. p. 594, 607, 609, 610. Scept. Chym. Abr. III. p. 281. Phil. Tranſ. Abr. II. p. 152. Merret. Exp. of Cold, p. 47, 48.

† Laſt winter I found good Burgundy, ſtrong claret, and Madeira, to freeze at about *gr.* 20.

‖ Boyle Exp. of Cold, Abr. I. p. 594.

‡ Tent. Exp. Ac. Cim. Add. p. 176.

But Mr. de Reaumur's rectified ſpirit of wine itſelf underwent in the winter at Torneao a cold greater than any thing he was able to make in France. There, too, it was frozen [†], though at what degree we cannot tell. Only we are ſure that the cold able to do it was no greater than —*gr.* 34; perhaps a good deal leſs. If people were apprized and on their guard, this might be accurately enough determined in ſuch cold countries; where I ſuppoſe, the moſt perfect alcohol might be frozen, and perhaps even the lately invented etherial ſpirit of wine itſelf.

Vinegar is the produce of a ſecond proceſs of fermentation, whereby all the oil and ſpirits of the wine are converted into a lean acid liquor, that reſiſts cold pretty much, and (like wine and ardent ſpirits) the ſtronger it is, the more. Common vinegar is indeed not difficult to freeze, that may ordinarily happen at about *gr.* 28. But when freed of a great deal of its phlegm by diſtillation, under the name of ſpirit of vinegar, it reſiſts a much greater cold. However, it was frozen when expoſed to an intenſe cold, as Mr. Boyle [‡] tells us;

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[*] Reaumur in Mem. Ac. Sc. 1734, p. 255.

[†] Mupert Fig. de la Terre, p. 38.

[‡] Exp. on Cold, Abr. I. p. 593.

as alſo when ſurrounded by an artificial mixture of ſnow and ſpirit of nitre. But Dr. Muſchenbroek*, who tried the experiment after Mr. Boyle†, neglected to obſerve, or at leaſt to tell us, the cold of that vinegar ice. In this freezing there was a part of it continued fluid; the ſtrongeſt and moſt concentrated vinegar that can well be imagined. And yet this too, by a new application of the cold mixture, was wholly turned into ice‡; whereby its ſtrength and acidity was conſiderably impaired.

But the ſtrong *mineral acid ſpirits* preſerve their fluidity much better; excepting, however, oil of vitriol, which begins congelation (or coagulation rather) near as ſoon as fair water ||. In the trials which Mr. Boyle ⊥ and Dr. Merret § made in the very cold weather, they found nothing of freezing in aqua fortis, ſpirit of nitre, ſpirit of ſalt, vitriol, &c. Nay, the ordinary cooling mixtures do not affect them. But Fahrenheit, by

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* Tent. Exp. Ac. Cim. Add. p. 175.

† Exp. on Cold, Abr. I. p. 590.

‡ Muſchen. ibid p. 176.

|| Merret Acc. of Freez. p. 8. Confer. & Boyle, Abr. I. p. 396, 559. III. p. 482. But it ſeems this its diſpoſition to freeze or coagulate goes off by time. See Muſchenbr. Eſſ. Phyſ. § 920.

⊥ Boyle Exp. of Cold, Abr. I. p. 596.

§ Merret Acc. of Freez. p. 17.

reiterating ſuch cold effuſions produced a very intenſe cold at—*gr.* 40, whereby ſpirit of nitre itſelf congealed and ſhot into cryſtals [*].

The alcaline ſpirits do not ſeem to be endowed with ſo great a power of reſiſting cold. Spirit of urine and ſpirit of human blood were frozen in cold weather in England [†]. And a cooling mixture of ſnow and ſpirit of nitre, congealed ſpirit of ſal ammoniac made with lime [‡]. This mixture was able to freeze ſpirit of vinegar, and weak ſpirit of ſalt, and to turn other ſaline liquors into figured ice. But it had no diſcernible influence on ſpirit of nitre.

Water impregnated with a moderate quantity of any of the ordinary ſalts freezes in cold weather; as with ſugar, alum, vitriol, nitre, ſea-ſalt, verdigreaſe, arſenic, ſal ammoniac, kelp, lixiviate ſalt, *&c.* [§]. But a ſolution of ſal gem did not freeze with the reſt [‖]. And oil of tartar *per deliquium* was brought to congeal only by a mixture of ſnow and ſalt [*]. Sea water freezes, whatever has been inadvertently ſaid to the contrary. The northern voyages are full of this [†]. The Zuyder Sea in Holland is often frozen up. And the Baltic is a ſtrong inſtance offering itſelf every year of the ſea water being converted into ice. Nay, it freezed at London with Mr. Boyle [‡]. And I have found ſmall parcels of ſaline bitter ice in ſome places of our ſea rocks, where the water had been toſſed up, and allowed to ſtagnate. The freezing point of ſea water is at *gr*.

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[*] See Boerh. Chem. I. p. 164, 165.

[†] Boyle Exp. on Cold. Abr. I. p. 593, 597. Exp. on Blood, III. p. 482.

[‡] Ibid. p. 590.

[§] Boyle Exp. on Cold, Abr. I. p. 590, 593, 596. Merret Ibid. p. 4, 5, 7.

[‖] Merret Ibid. p. 7, 8, 47.

But water, when fully ſaturated with thoſe ſalts, doth not eaſily freeze [§]. That ſuch ſolutions reſiſted the colds in England, will not be wondered at. But we are told that a ſtrong brine of bay ſalt cannot be frozen even in the northern countries [‖]. And ſyrups, which are but ſtrong ſolutions of ſugar and water boiled up together, are not congealed with any ordinary cold [**].

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[*] Boyle Ibid. p. 593, 596.

[†] See Muſſchenbr. Eſſ. Phyſ. § 925.

[‡] Exp. of Cold, p. m. 147.

[§] Boyle Exp. on Cold, Abr. I. p. 597. Merret ib. p. 6.

[‖] Phil. Tranſ. Abr. II. p. 152.

[**] Merret Ibid. p. 52, 53.

Of Oils.

Oils too may be frozen, or at leaſt coagulated by cold. And ſome of them, as the reſins, common ſulphur, ſpermaceti, tallow, butter, *&c.* exiſt ordinarily in a conſiſtent form, though in their nature they are as truly oils as thoſe that moſt obſtinately retain their fluid ſtate. But as oils are gradually thinned by heat or thickened by cold, the limits between their conſiſtency and fluidity are not ſo exactly determinable in them as in watry ſubſtances.

Very many oils preſerve their fluidity in great colds [*]. We have not heard of the ordinary diſtilled oils being frozen; except that once Mr. Boyle [†] found that unrectified oil of turpentine had its upper part turned to ice. And oil of aniſe for ordinary appears in a ſort of butter ſtate till it be warmed to *gr.*

Even many of the expreſſed oils ſtand out the cold very well; ſuch as oils of lintſeed, hempſeed, nuts, ſweet almonds, *&c.* [‡]. But common oil of olives ſtiffens and grows opaque very ſoon, as at about *gr*. 43. But different oils, and different parts of the ſame oil freeze at different degrees of heat. So that we ſee ſome part of this oil ſometimes continue fluid in pretty ſtrong froſts.

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[*] See Boyle Exp. on Cold, Ab. I. p. 596, 597. Orig. of Heat, &c. Abr. I. p. 559.

[†] Ibid. p. 597.

[‡] Boyle Orig. of Heat, &c. Abr. p. 559. Exp. on Cold, Abr. I. p. 598.

Wax is a vegetable oil collected from flowers by the bees. This ſwims on water, and does not loſe its firmneſs and opaqueneſs till that be heated to *gr*. 142.

All the reſins may be looked on too as vegetable oils, and they melt with a heat below that of warm water. Common roſin comes to a ſort of fluid by the heat of about *gr*.

Many of the mineral oils appear for ordinary fluid, as petroleum, naphtha, *&c.* nor are we yet informed at what cold they become firm. Others again, as aſphaltos, ſuccinum, *&c.* we find always in a firm dry form; but which may be melted by heat. And ſulphur too is ſuch an oil. And flowers of brimſtone, that is to ſay, pure ſulphur, melts into a fluid with the heat of *gr*.

Train oil does not eaſily freeze *. And I ſuppoſe it was from this that Olaus Magnus † tells us, that it is uſual in the northern regions to fill up the frozen ditches of fortified places with train oil in the winter, to preſerve the lower water fluid.

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* Boyle Exp. on Cold, Abr. I. p. 596, 598.

† Vid. Ibid. p. 596.

But Mr. Boyle* and Dr. Merret† found it frozen even by the cold weather of England.

But the other ordinary animal oils rather take a conſiderable heat to keep them fluid. Butter is ſuch an oil And that in the month of May took a heat between *gr* 80 and 90 to oil it: but being ſo heated, it keeps ſomething thin till it fall to about *gr*. 74.

Hogs lard.

Tallow.

Spermaceti is the firmer part of the oil of the Macrocephalus or Dentatus whale; and does not melt with a heat under *gr*.

Of Salts.

We lately propoſed common water as a taſteleſs ſalt diſſolvable by a ſmall degree of heat. What if, on the other hand, we ſhould reckon the ordinary ſalts as different waters brought to a liquid ſtate by a greater degree of fire, ſome ſooner, ſome later?

Alum melts into a fluid at *gr*.

Sea ſalt.

Borax.

Nitre.

Sal ammoniac.

Strong alcali ſalt.

Vitriol.

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[*] Boyle Exp. on Cold, p. 596.

[†] Merr. Ibid. p. 47.

VII. *Of*

VII. *Of the melting and ſhining Heats of Metals and Minerals.*

And now are we come to conſider thoſe, which of all terreſtrial bodies, except elementary earth itſelf, are the moſt conſiſtent and firm, and moſt ſuſceptible of the greateſt heats; I mean metals and mineral ſubſtances. They are naturally in a conſiſtent form: but ſome of them flow at a moderate enough fire; while others require ſo intenſe a heat to diſſolve them, that we find it difficult how to bring it to a calcul.

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Quickſilver is always reckoned in the claſs of metals. But it is ſo far from the common conſiſtent form and appearance of other metals, that it may juſtly be looked on as the trueſt, viſible, and tangible fluid in the world. By ſome ſlight additions it may indeed be robbed of that property. But when left to itſelf pure and unmixed, it always preſerves its fluidity in the very greateſt colds natural or artificial that have ever been obſerved *.

* See Boyle Exp. on Cold, Abr. I. p. 597. Boerh. Chem. I. p. 36, 165. De Maupert. Fig. de la Terre, p. 58.

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Of all the ordinary ſimple metals or metallic ſubſtances *tin* loſes its conſiſtency, and flows the eaſieſt, or with the loweſt heat. By Sir Iſaac Newton's obſervation [*], reduced to the numbers of our Thermometer, it melts at *gr.* $\left(\frac{71\times180}{34}+32=\right)408$. Dr. Muſſchenbroek's experiments [†] carried it but a little higher, *viz.* to *gr.* $\left(\frac{100\times180}{53}+32=\right)422$. And there may be greater differences from accidental mixtures. Sir Iſaac's experiments of moſt of the heats below this of tin are, I believe, pretty near the truth. The heats beyond this being deduced in a more precarious way, we cannot be ſo ſure of.

Lead is the metal next to tin that fluxes eaſieſt. But here I do not find different folks obſervations agreeing ſo well as in the other. In Sir Iſaac's way of finding out the heat of lead beginning to melt, that would fall on our *gr.* 540 [‡]. But according to Muſſchenbroek's experiment [§] reduced to our numbers, the mercury by ſuch a heat ſhould riſe to *gr.* 769; were it ſuſceptible of ſuch a great expanſion in a regular manner. And Mr. Amontons[*] found gun-powder to be kindled by the ſame heat that melted lead.

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[*] Phil. Tranſ. Abr. IV. 2. p. 2, 4.

[†] Tent. Exp. Ac. Cim. Add. II. p. 21.

[‡] Ibid. p. 2.

[§] Ibid.

Some bodies, by being combined into one maſs, become more firm than either were in a ſeparate ſtate. And, on the other hand, others, when put together, make a compound more diſſolvable than its ingredients; much contrary to our expectations. So little ſhould we pretend *a priori* and without experiments to determine the nature and properties of bodies.

Thus lead and tin may be mixed together in ſuch proportions, as that the whole ſhall melt more eaſily than even tin itſelf[†]. A compound of three parts tin and two of lead Sir Iſaac found to melt at his *gr.* 57, or our *gr* $\left(\frac{57\times180}{34}+32=\right)334$[‡]. But if the lead be in a greater proportion to the tin, a higher heat than that of melting tin is requiſite to make the compound flow. For he experienced a heat of *gr.* 46,0 to be required to melt a mixture of four parts lead and one part tin[‖]. When the lead was to the tin as 5 to 1, that heat was not able to keep the mixture in fuſion[§].

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[*] Mem. Ac. Sc. 1703. p. 247.

[†] Muſſchenb. Ibid. Boerh. Chem. I. p. 751.

[‡] Phil. Tranſ. Abr. IV. 2. p. 2.

[‖] Ibid.

[§] Ibid.

What ſtill vaſtly increaſes the fuſibleneſs of theſe metals is the addition of biſmuth or tin-glaſs, a marcaſite or ſulphureo-metallic ſubſtance, not ſo eaſily melted as tin. That biſmuth Sir Iſaac Newton uſed in theſe curious experiments, we ſhall juſt now have occaſion to copy from him, did not melt with a heat under *gr.* 460[*]. And Dr. Muſſchenbroek's biſmuth was much harder to diſſolve, to wit, at *gr.* 1051[†]. And a golden marcaſite, though more fluxil, took *gr.* 506 to melt it.

A mixture of tin and tin-glaſs melts at a heat leſs than what is requiſite to melt either ſeparately[‡]. When they were in equal quantities, the compound melted at *gr.* 283[||]. If you alter this proportion, the mixture becomes ſuſceptible of a greater heat. When the tin was double the biſmuth, it did not melt with a heat under *gr.* 334[§]. And it ſtiffened in this heat when the tin was to the biſmuth as 5 to 3[**]. And if the proportion of biſmuth be very ſmall, the heat required to keep the compound in fuſion is ſtill greater, and comes nearer to that of melting tin. When there was eight times more tin than biſmuth in the mixture, it would not melt with a heat under *gr.* 392[*].

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[*] Phil. Tranſ. Abr. IV. 2. p. 2.

[†] Muſſchenbr. Ibid.

[‡] Ibid.

[||] Newton, Ibid. p. 2.

[§] Ibid.

[**] Ibid.

We are told by Sir Iſaac that the heat of melting lead was *gr.* 540, and that of biſmuth 460. But theſe two bodies, ſo difficult to be melted by themſelves, when blended together into one maſs, are much eaſier brought into fuſion. A mixture of them in equal quantities but juſt ſtiffened at *gr.* 334[†]; and ſo would melt with very little more heat.

But what is moſt ſurpriſing of all is the great fuſibleneſs of a mixture of all theſe three ingredients. Mr. Homberg[‡] propoſed for an anatomical injection a compound metal of lead, tin, and biſmuth in equal parts, which he tells us keeps in fuſion with a heat ſo moderate that it would not ſinge paper. Sir Iſaac contrived a mixture of them in ſuch proportions that it melted and kept fluid with a heat ſtill ſmaller, not very much exceeding that of boiling water. A compound of two parts lead, three parts tin, and five of biſmuth but juſt ſtiffened at that very heat[§]; and ſo would have fluxed with very little more. And another mixture of the ſame materials, in a proportion ſomewhat different from that, actually melted by the heat of *gr.* 246 *. It conſiſted of the lead, tin, and biſmuth, in the proportions to one another, of 1, 4, and 5.

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[*] Newton, Ibid. p. 2.

[†] Idem, Ibid.

[‡] Mem. Ac. Sc. 1699. p. 235.

[§] Newton, Ibid.

Antimony is a mineral compound of ſulphureous and metallic parts The metallic ſubſtance when ſeparated from the other is called its *Regulus*. This is able to bear a great heat without melting. When prepared with iron, and ſo going under the name of *Regulus Martis*, it was found to ſtiffen only at *gr.* 805 †, and ſo would take a heat ſomething greater thoroughly to melt it.

But this regulus fluxes ſomething eaſier when mixed with tin or biſmuth. A mixture of it and tin in equal portions was ſound to melt at *gr.* 635 ‡. When the regulus exceeded five times the quantity of tin, it ſtiffened at *gr.* 752 ‖. And ſo too did a mixture of regulus and biſmuth in the proportions of two to one §. But when the regulus was in lower proportion to the biſmuth, as in the ratio of four to ſeven, the compound was eaſier melted; for it ſtiffened at *gr.* 635 ⸸.

Though it would ſeem by theſe experiments that the metal of antimony gave the tin and tin-glaſs a power of reſiſting the fire, and that even ſomething greater than its proportional quantity in the mixtures ſhould require; yet it promotes the fluxibility of the harder metals. When added to red-hot iron it ſoon brings it into fuſion *. And it increaſes the fuſibleneſs and volatility of ſome of the other metals †.

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* Newton, Ibid.

† Idem, Ibid. p. 3.

‡ Idem, Ibid.

‖ Idem, Ibid.

§ Idem, Ibid.

⸸ Idem, Ibid.

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The *hard metals*, ſilver, gold, copper, and iron, require a more intenſe degree of heat to deſtroy their conſiſtency, and bring them into a fluid ſtate. And before that, too, they undergo a very remarkable change, appearing in a ſhining form; when we know that they have a vaſt burning force, far exceeding the heat of ordinary hot bodies, as the ſummer air, boiling water, *&c.* the proportions of which ſome have endeavoured to determine.

Dr. Keill ‡ affirms “ the heat of red-hot iron to “ be only ſeven times greater than the ordinary heat “ of the ſun in ſummer.” But this ordinary heat of the ſun in ſummer is not a very definite thing, eſpecially as then underſtood. So that we are not quite ſure what Dr. Keill really reckoned to be the heat of red-hot iron. Though I ſuppoſe he had ſome view to what he found in Sir Iſaac Newton on that ſubject.

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* Boerh. Chem. I. p. 55. II. p. 511.

† Ibid. I. p. 34, 38.

‡ Exam. of Burn. Theor. p. 156.

Very early had this great man been making experiments on the various degrees of heat in bodies. And it was from theſe his firſt trials he had what he ſays of them in his *Principia*, reckoning the heat of red-hot iron to be three or four times greater than that of boiling water*: almoſt as Dr. Pitcairn ſoon after gave out the matter in his colleges at Leyden†, to whom I am ready to think Sir Iſaac had communicated this obſervation, that "by the heat "of our ſummer air the oil is raiſed in the Ther-"mometer $7\frac{1}{2}$ degrees; by the heat of our ſkin to "*gr.* 17; by boiling water to *gr.* 50 or 52; by the "heat of red-hot iron to thrice that number," or to *gr.* $(180\times3+32=)572$ in our ſcale.

But upon what grounds Sir Iſaac reckoned red-hot iron (*ferrum candens*) to be but three or four times hotter than boiling water, I cannot well tell. Nor how either he or Dr. Pitcairn could determine the dilatation of lintſeed oil by the direct application of the heat of red-hot iron, can I conceive any way poſſible; for it boils by a much leſs heat than that. And, if you were to imagine oil to expand regularly as far as you pleaſe without boiling, the heat of red-hot iron would carry it much beyond 150 or 200 ſuch degrees; that is to ſay, it would dilate the oil much more than three or four times the expanſion given it by boiling water.

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* Princip. p. 508.

† Elem. Med. II. 1. § 26.

It is true Sir Iſaac gives out this computation only as a conjecture, which, however, as it ſtands in the firſt edition, is ſtill preſerved without the leaſt alteration in both the other two: though he had, long before he publiſhed them, made ſome new experiments, whereby in a very ingenious way he had found the heat of red-hot iron to be a good deal higher: for his firſt inquiries concerning the comparative heat of bodies muſt (as is commonly the fate of all firſt eſſays) have laboured under ſome imperfection. But, as this affair was well worth proſecuting, his great and inquiſitive genius did not reſt in them. He repeated his trials with greater care and exactneſs, and *an.* 1701 gave in to the Royal Society the reſult of them in that excellent paper, (to which already we have been ſo much beholden for ſo many curious experiments and uſeful hints), containing *a ſcale of the degrees of heat* determined with an oil Thermometer and a lump of red-hot iron[*].

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[*] Phil. Tranſ. Abr. IV. p. 1, *&c.*

Of this method of determining the degrees of heat we have already[*] had occaſion to ſpeak more fully. It is the reſult of his obſervations we are juſt now concerned with. And we are told[†], that at his *gr*. 114, which coincides with our *gr*. $\left(\frac{114\times180}{34}+32=\right)635$ heated bodies begin to ſhine in the dark, but juſt ſo as to be perceptible. This may be called the *ſhining point of heat*. But I would ſuppoſe, from the train of Sir Iſaac's experiments, that this, and what he ſays farther of the ſplendour of heated bodies, is to be meant ſolely of iron: for it is likely enough, that different metals and other bodies may arrive at their ſhining heats in different degrees.

But iron muſt be vaſtly heated beyond this to have the character of being red hot. Before it can ſhine briſkly even in the dark, it muſt be at *gr*. 752[‡]: and yet then it has no perceptible ſplendour in the twilight. And it is not till the heat arrive at *gr*. 884, that bodies ſhine diſtinctly in the twilight immediately before ſun-riſing or after its ſetting[‖], but not at all or very obſcurely in ſtrong day-light: and, agreeable hereto, we find among

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[*] Eſſ. on the Heating and Cooling of Bodies, § 3, *&c.*

[†] Phil. Tranſ. ibid. p. 3.

[‡] Ibid.

[‖] Ibid.

Dr. Muſſchenbroek's experiments [*] an iron rod heated, but not reddened in the fire to have been lengthened 276 deg. of his Pyrometer; which falls on *gr.* $\left(\frac{276\times180}{53}+32=\right)969$ in our ſcale.

In Sir Iſaac's reckoning the heat of a ſmall pit-coal fire unblown, and of iron heated red-hot in it, was *gr.* $\left(\frac{192\times180}{34}+32=\right)1049$ [†]: and a rod of ſteel heated in the fire till it was red, Dr. Muſſchenbroek [‡] found to be lengthened 364 of his degrees, and conſequently to have been heated to our *gr.* $\left(\frac{364\times180}{56}+32=\right)1095$, you ſee not differing much from Sir Iſaac's heat of a ſmall pit-coal fire.

But he [||] gives out the heat of a ſmall wood-fire as greater, amounting perhaps to his *gr.* 200 or 210, which is our *gr.* $\left(\frac{210\times180}{34}+32=\right)$ 1408: and a greater fire he juſtly reckons hotter ſtill, eſpecially if blown with bellows. Copper ſhining hot from the fire Muſſchenbroek [**] lengthened 392 diviſions, that is, to our *gr.* $\left(\frac{302\times130}{59}+32=\right)$ 1228.

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[*] Tent. Exp. Ac. Cim. Add. II. p 47, 48.

[†] Phil. Tranſ. ibid.

[‡] Tent. *&c.* ibid. p. 48, 49.

[||] Phil. Tranſ. ibid.

[**] Tent. *&c.* ibid. p. 49, 50.

And, to be ſure, iron, in reddening and melting, takes on a greater heat than copper.

This matter to be farther proſecuted by experiments.

VIII. *Of the boiling Heats of liquid and melted Bodies.*

A very moderate degree of heat, no greater than that of hot animals, is ſufficient to generate air bubbles in freſh *water.* But, to make it really boil, a vaſtly greater heat is required. By the very conſtruction of our Thermometers that happens when the mercury is raiſed to *gr.* 212; beyond which, water is incapable of being heated in an ordinary ſtate of the atmoſphere: which circumſtance is to be underſtood, too, of the *boiling heats* of the other fluids we ſhall have occaſion to mention.

Sea water in boiling does not take on a heat ſenſibly greater than common water: but, if the water be as ſtrongly impregnated with ſalt as it is capable of being, it is ſuſceptible of ſome more heat. Dr. Boerhaave* found ſuch a ſtrong pickle boil up to the heat of *gr.* 218.

A *lixive of pot-aſhes* is heated to *gr.* 240 before it boils †: and the common *ſaline ſpirits,* which are ſtrong ſolutions of volatilized acid ſalts in water, bear this great or a greater degree of heat. Spirit of nitre comes to *gr.* 242 [*]; and I ſuppoſe aqua fortis, ſpirit of ſalt, *&c.* are ſuſceptible of much the ſame. But oil of vitriol, that ſtrongeſt of all the acid ſpirits, (and oil of ſulphur *per campanam* is much akin to it), goes to a vaſtly greater heat, *viz.* to *gr.* 546 [†].

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* Chem. I. p. 746.

† Phil. Tranſ. Abr. VI. 2. p. 18.

But the common true *oils* require ſtill a much greater heat to bring them to a boiling. It is true *alcohol* boils with the loweſt degree of heat of any liquor, to wit, at *gr.* 174 or 175 [‡], and brandy at about *gr.* 190, as I have experienced: and alcohol is but a thin oil; but then, by the proceſſes of fermentation and diſtillation, it is ſo much ſubtilized and attenuated, as to become thinner, more volatile, and eaſier boiled than water itſelf, as Lord Verulam too, long ago, obſerved [||].

Thoſe oils that have any lentor in them have a much ſtronger coheſion of parts, and can bear a much greater heat. "Majorem calorem deſiderat "ad hoc ut bulliat oleum, quàm aqua; & tar-"dius multo bullire incipit," ſays the ſame Lord

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[*] Phil. Tranſ. Abr. VI. 2. p. 18.

[†] Ibid.

[‡] Ibid. and Boerh. Chem. I. p. 168, 745, 746, 749.

[||] Hiſt. Denſ. *&c.* p. 46.

Verulam *. Oil of turpentine is amongſt the thinneſt of all theſe, and yet in boiling it raiſed the mercury in the Thermometer almoſt to *gr.* 560 †: and hereabout I reckon the boiling heat of the other ordinary volatile or ſtillatitious oils; which, however, as the boiling continues, is always augmenting, the more volatile parts flying away, and leaving the reſidue thicker, ſtronger, and ſuſceptible of greater heat ‡.

The *expreſſed fixed oils* can take on ſtill a greater heat. I find Mr. Fahrenheit and Dr. Boerhaave ſpeaking as if they and quickſilver began to boil at much the ſame degree of heat, *viz.* about *gr.* 600 ‖: and in boiling they are ſuſceptible of ſtill a much greater heat than this before they burſt out into flame. Dr. Muſſchenbroek has furniſhed us with a method of judging of that heat, independently of the common Thermometers, from the lengthening of metals expanded in it, by which the heat of boiling oil came out conſiderably higher than what is aſſigned to mercury boiling. From the freezing point he found ** the elongation of an iron rod in rapeſeed oil ſo boiling as to be ready to burſt into ſlame to its elongation in boiling water, as 201 to 53, and conſequently as 682 to 180: ſo that, if the mercurial Thermometer had been capable of being lengthened out far enough, this boiling oil would have raiſed the mercury in it to *gr.* $682+32=714$; and that is a heat in the ordinary way of reckoning from above the freezing point, $(\frac{682}{180}=3\frac{4}{5})$, near four times greater than that of boiling water.

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* Hiſt. Denſ. &c. p. 46.

† Boerh. Chem. I. p. 747.

‡ Ibid. and p. 748. and Phil. Tranſ. ibid.

‖ Fahr. in Phil. Tranſ. ibid. p. 18, 52. Boerh. ibid. p. 165, 265, 291, 747, 748, 752. Muſſchenbr. Eſſ. Phyſ. § 880, 952, 896.

** Tent. Exp. Ac. Cim. Add. II. p. 20.

It is true, a much lower heat made a ſort of boiling noiſe and froth in the oil, ariſing from the expulſion of its more watry parts. This came on when the iron rod was lengthened but 120 parts, and that lights about *gr.* 440 in our Thermometer. But this is but an accidental and indefinite ſort of heat, and not at all to be reckoned the complete boiling ſtate of the oil, or the greateſt heat it is capable of without burning.

FINIS.

ly high-